PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

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FOR THE YEAR MDCCCXLIX.

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MDCCCXLIX.

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THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body,

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upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

The Meteorological Journal hitherto kept by the Assistant Secretary at the Apartments of the Royal Society, by order of the President and Council, and published in the Philosophical Transactions, has been discontinued. The Government, on the recommendation of the President and Council, has established at the Royal Observatory at Greenwich, under the superintendence of the Astronomer Royal, a Magnetical and Meteorological Observatory, where observations are made on an extended scale, which are regularly published. These, which correspond with the grand scheme of observations now carrying out in different parts of the globe, supersede the necessity of a continuance of the observations made at the Apartments of the Royal Society, which could not be rendered so perfect as was desirable, on account of the imperfections of the locality and the multiplied duties of the observer.

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HER MAJESTY QUEEN VICTORIA, in restoring the Foundation of the Royal Medals, has been graciously pleased to approve the following regulations for the award of them:

That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

That the triennial Cycle of subjects be the same as that hitherto in operation: viz.

1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology or Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the year 1849 for the most important paper in Physics, communicated to the Royal Society after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848, and printed in the Philosophical Transactions.

The Council propose also to give one of the Royal Medals in the year 1849 for the most important paper in Geology or Mineralogy, communicated to the Royal Society after the termination of the Session in June 1845, and prior to the termination of the Session in June 1848, and printed in the Philosophical Transactions.

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The Council propose to give one of the Royal Medals in the year 1851 for the most important paper in Astronomy, communicated to the Royal Society after the termination of the Session in June 1847, and prior to the termination of the Session in June 1850, and printed in the Philosophical Transactions.

The Council propose also to give one of the Royal Medals in the year 1851 for the most important paper in Physiology, including the Natural History of Organized Beings, communicated to the Royal Society after the termination of the Session in June 1847, and prior to the termination of the Session in June 1850, and printed in the Philosophical Transactions.

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The Council propose also to give one of the Royal Medals in the year 1852 for the most important paper in Geology or Mineralogy, communicated to the Royal Society after the termination of the Session in June 1848, and prior to the termination of the Session in June 1851, and printed in the Philosophical Transactions.

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PHILOSOPHICAL TRANSACTIONS.

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Received October 4,—Read December 7, 1848.

§ 28. *On the crystalline polarity of bismuth and other bodies, and on its relation to the magnetic form of force.*

¶ i. *Crystalline polarity of bismuth.* ¶ ii. *Crystalline polarity of antimony.* ¶ iii. *Crystalline polarity of arsenic.*

2454. MANY results obtained by subjecting bismuth to the action of the magnet have at various times embarrassed me, and I have either been contented with an imperfect explanation, or have left them for a future examination: that examination I have now taken up, and it has led to the discovery of the following results. I cannot, however, better enter upon the subject than by a brief description of the anomalies which occurred, and which may be obtained at pleasure.

2455. If a small open glass tube have a bulb formed in its middle part and some clean good bismuth be placed in the bulb and melted by a spirit-lamp, it is easy afterward, by turning the metal into the tubular part of the arrangement, to cast it into long cylinders: these are very clean, and when broken are seen to be crystallized, usually giving cleavage planes, which run across the metal. I prepare them from 0·05 to 0·1 of an inch in diameter, and, if the glass be thin, usually break both it and the bismuth together, and then keep the little cylinders in their vitreous cases.

2456. Taking some of these cylinders at random and suspending them horizontally between the poles of the electro-magnet (2247.), they presented the following phe-

nomena. The first pointed axially; the second, equatorially; the third, equatorial in one position, and obliquely equatorial if turned round on its axis 50° or 60°; the fourth, equatorially and axially under the same treatment; and all of them, if suspended perpendicularly, pointed well, vibrating about a final fixed position which seemed to have no reference to the form of the cylinders. In all these cases the bismuth was strongly diamagnetic (2295, &c.), being repelled by a single magnetic pole, or passing off on either side from the axial line between two poles. A similar piece of finely-grained or granular bismuth was, under the same circumstances and at the same time, affected in a perfectly regular manner, taking up the equatorial position (2253.), as a body simply diamagnetic ought to do. The cause of these variations was finally traced to the regularly crystalline condition of the metallic cylinders.

¶ i. *Crystalline polarity of bismuth.*

2457. Some bismuth was crystallized in the usual manner by melting it in a clean iron ladle, allowing it partly to congeal, and then pouring away the internal fluid portion. Pieces so obtained were then broken up by copper hammers and tools, and groups of the crystals separated, each group or piece consisting only of those crystals which were symmetrically arranged, and therefore likely to act in one direction. If any part of the fragments had been in contact with the iron ladle, it was cleared away by rubbing on sandstone and sand-paper. Pieces weighing from 18 grains to 100 grains were thus easily obtained.

2458. The electro-magnet employed in the first instance was that already described (2247.), having moveable terminations which supplied either conical, round, or flat-faced poles. That the suspension of the bismuth might be readily effected and unobjectionable as to magnetic influence, the following arrangement was generally adopted. A single fibre of cocoon silk, from 12 to 24 inches in length, was attached to a fit support above, and made fast below to the end of a piece of fine, straight, well-cleaned copper wire, about 2 inches in length; the lower end of this wire was twisted up into a little head, and then furnished with a pellat of cement, made by melting together a portion of pure white wax, with about one-fourth its weight of Canada balsam. The cement was soft enough to adhere by pressure to any dry substance, and sufficiently hard to sustain weights up to 300 grains, or even more. When prepared, the suspender was subjected by itself to the action of the magnet, to ascertain that it was free from any tendency to point, or be affected; without which precaution no confidence could be reposed in the results of the experiments.

2459. A piece of selected bismuth (2457.), weighing 25 grains, was hung up between the poles of the magnet, and moved with great freedom. The constituent cubes were associated in the usual manner, being attached to each other chiefly in the line joining two opposite solid angles; and this line was in the greatest length of the piece. The instant that the magnetic force was on, the bismuth vibrated strongly

about a given line, in which, at last, it settled; and if moved out of that position it returned, when at liberty, into it; pointing with considerable force, and having its greatest length *axial*.

2460. Another piece was then selected, having a flatter form, which when subjected to the magnetic power, pointed with the same facility and force, but its greatest length was equatorial: still the line according to which the cubes tended to associate diametrally, was, as before, in the *axial* direction. Other pieces were then taken of different forms, or shaped into various forms by rubbing them down on stone, but they all pointed well; and took up a final position, which had no reference to the shape, but was manifestly dependent on the crystalline condition of the substance.

2461. In all these cases the bismuth was diamagnetic, and strongly repelled by either magnetic pole, or from the axial line. It was affected only whilst the magnetic force was present. It set in a given constant position perfectly determinate; and, if moved, always returned to it, unless the extent of motion was above 90°, and then the piece moved further round and took up a new position diametrically opposed to the former, which it then retained with equal force, and in the same manner. This phenomenon is general in all the results I have to refer to, and I will express it by the word diametral:—diametral set or position.

2462. The effect occurs with a single magnetic pole, and it is then striking to observe a long piece of a substance, so diamagnetic as bismuth, repelled, and yet at the same moment set round with force, axially or end on, as a piece of magnetic substance would do.

2463. Whether the magnetic poles employed (2458.) are pointed, round, or flat-faced, still the effect on the bismuth is the same: nevertheless, the form of the poles has an important influence of a subordinate kind; and some forms are much more fitted for these investigations than others. When pointed poles are employed, the lines of magnetic force (2149.) rapidly diverge, and the force itself diminishes in intensity to the middle distance from each pole. But when flat-faced poles are used, though the lines of power are curved and vary in intensity at and towards the edges of the flat faces, yet there is a space at the middle of the magnetic field where they may be considered as parallel to the magnetic axes, and of equal force throughout. If the flat faces of the poles be square or circular, and their distance apart about one-third of their diameter, this space of uniform power is of considerable extent. In my experience the central or axial portion of the magnetic field is sensibly weaker than the circumjacent parts; but, then, there is a small screw-hole in the middle of each pole face, for the attachment of other forms of termination.

2464. Now the law of action of bismuth, as a *diamagnetic* body, is, that it tends to go from stronger to weaker places of magnetic force (2267. 2418.); but as a *magnecrystallic* body it is subject to no effect of the kind; and is as powerfully affected by lines of equal force as by any other. So a piece of amorphous bismuth, suspended in a magnetic field of uniform power, seems to have lost its diamagnetic force alto-

gether, and tends to acquire no motion but what is due to torsion of the suspending fibre, or currents of air: but a piece of regularly crystallized bismuth is, in the same situation, very powerfully affected by virtue of its magnecrystallic condition.

2465. Hence the great value of a magnetic field of uniform force; and, if, hereafter, in the extension of these investigations to bodies having only a small degree of crystalline power, a perfectly uniform field should be required, it could easily be given by making the form of the pole face somewhat convex, and rounded at the edges more or less. The required shape could be ascertained by calculation, or perhaps better in practice, by the use of a little test cylinder of bismuth in the granular or amorphous state, or of phosphorus.

2466. In addition to these observations it may be remarked, that small crystals, or masses of crystals, and such as approach in their general shape to that of a cube or a sphere, are better than large or elongated pieces; inasmuch, as if there be irregularities in the force of a magnetic field, such pieces are less likely to be affected by them.

2467. When the crystal of bismuth is in a magnetic field of equal strength, it is equally affected whether it be in the middle of the field or close up to one or the other magnetic pole; *i. e.* the number of vibrations in equal times appears to be equal. Much care, however, is required in estimating it by such means, because, from the occurrence of two positions of unstable equilibrium in the equatorial direction, the vibrations in large arcs are much slower than those in small arcs; and it is difficult in different cases to adjust them to the same extent of vibration.

2468. Whether the bismuth be in a field of intense magnetic force or one of feeble powers; whether the magnetic poles are close up to the piece, or are opened out until they are five or six inches or even a foot asunder; whether the bismuth be in the line of maximum force, or raised above, or lowered beneath it; whether the electric current be strong or weak, and the magnetic force, therefore, more or less in that respect; if the bismuth be affected at all it is always affected in the same manner.

2469. The results are, altogether, very different from those produced by diamagnetic action (2418). They are equally distinct from those dependent on ordinary magnetic action. They are also distinct from those discovered and described by PLÜCKER, in his beautiful researches into the relation of the optic axis to magnetic action; for there the force is equatorial, whereas here it is axial. So they appear to present to us a new force, or a new form of force in the molecules of matter, which, for convenience sake, I will conventionally designate by a new word, as the *magnecrystallic* force.

2470. The direction of this force is, in relation to the magnetic field, *axial* and *not equatorial*; this is proved by several considerations. Thus, when a piece of regularly crystallized bismuth was suspended in the magnetic field, it pointed; keeping it in this position, the point of suspension was removed 90° in the equatorial plane (2252.), so that when again freely suspended, the line through the crystal,

which was before horizontal in the equatorial plane, was now vertical; the piece again pointed, and generally with more force than before. The line passing through the crystal, coincident with the magnetic axis, may now be taken as the line of force; and if the process of a quarter revolution in the equatorial plane be repeated, however often, the crystal still continues to point with the assumed line of force in the magnetic axis, and with a maximum degree of power. But now, if the point of suspension be removed 90° in the plane of the axis, *i. e.* to the end of the assumed line of force, so, that when the crystal is again freely suspended this line is vertical; then, the crystal presents its peculiar effect at a *minimum*, being almost or entirely devoid of pointing power, and exhibits in relation to the magnet, only the ordinary diamagnetic force (2418.).

2471. Now if the power had been equatorial and polar, its maximum effect would not have been produced by a change of the point of suspension through 90° in the equatorial plane, but by the same change in the axial plane, and any similar change after that in the axial plane, would not have disturbed the maximum force; whereas a single change of 90° in the equatorial plane, would have brought the line of force vertical (as in PLÜCKER's case of Iceland spar), and reduced the results to a minimum or zero.

2472. The directing force, therefore, and the set of the crystal are in the *axial* direction. This force is, doubtless, resident in the particles of the crystal. It is such, that, the crystal can set with equal readiness and permanence in two diametral positions: and that between these there are two positions of equatorial equilibrium, which are, of course, unstable in their nature. Either end of the mass or of its molecules, is to all intents and purposes, both in these phenomena, and in the ordinary results of crystallization, like the other end; and in many cases, therefore, the words *axial* and *axiality* would seem more expressive than the words *polar* and *polarity*. In presenting the ideas to my own mind, I have found the meaning belonging to the former words the most useful.

2473. On placing the *metal* in other positions, and therefore in a constrained condition, no alteration of the state or power of the bismuth, either in force or direction, is produced by the power of the magnet, however strong its enforcement or long its continuance.

2474. It is difficult readily to describe the position of this force in relation to the crystal, though most easy to ascertain it experimentally. The form of the bismuth crystals is said to be that of a cube, and of its primitive particle a regular octohedron. To me the crystals do not seem to be cubes, but either rhomboids or rhombic prisms, approaching very nearly to cubes. My measurements were very imperfect and the crystals not regular; but as an average of several observations, the planes were inclined to each other at angles of $91\frac{1}{2}°$ and $88\frac{1}{2}°$; and the boundary lines of a plane at $87\frac{1}{2}°$ and $92\frac{1}{2}°$. Whatever be the true form, it is manifest upon inspection, that the aggregating force tends to produce crystals having more or less of the rhomboidal

shape and rhombic planes; and that these crystals run together in symmetric groups, generally in the direction of their longest diameters. Now the line of *magnecrystallic* force almost always coincides with this direction where the latter is apparent.

2475. The *cleavage* of bismuth crystals removes the solid angles and replaces them by planes; so that there are four directions producing the octohedron. These cleavages are not (in my experience) made with equal facility, nor do they produce planes equally bright and perfect. Two, and more frequently one, of these planes is more perfect than the others; and this, the most perfect plane, is that which is produced at the most acute solid angle (2474.); and is generally easily recognized. When a bismuth crystal presents many planes of cleavage and is suspended in the magnetic field, one of these planes faces towards one of the magnetic poles, and its corresponding plane, if it be there, towards the other; so that the line of magnecrystallic force is perpendicular to this plane: and this plane corresponds to the one which I have already described as being, generally, the most perfect, and replacing the acute angle of the crystal.

2476. A single crystal of bismuth was selected and cut out from the mass by copper tools, and the places where it had adhered were rubbed down on sand-paper, so as to give the fragment a cube-like form with six planes; four of these planes were natural. One of the solid angles, expected to be that terminating or in the direction of the line of magnecrystallic force, was removed, so as to expose a small cleavage plane, which was bright and perfect, as also was expected. When suspended in the magnetic field with this plane vertical, the crystal instantly pointed with considerable force, and with the plane towards either one or the other magnetic pole; so that the magnecrystallic axis appeared now to be horizontal and acting with its greatest power. When this axial line was made vertical, and the plane therefore horizontal, the position being carefully adjusted, the crystal did not point at all. Being now suspended in succession at all the angles and faces of the cube, it always pointed with more or less force; but always so that a line drawn perpendicularly through the indicating cleavage plane (representing therefore the line of force) was in the same vertical plane as that including the magnetic axis: and, finally, when the bright cleavage plane was horizontal and the line of directive force therefore vertical, inclining it a little in a given direction, would make any given part of the crystal point to the magnetic poles.

2477. A *group* of bismuth crystals, the apex of which was terminated by a single small cleavage facet, was found to give the same results.

2478. Occasionally groups of crystals (2457.) occurred which did not seem capable of being placed in some one position in which they lost all directive power, but seemed to retain a minimum degree of force. It is very unlikely, however, that all the groups should be perfectly symmetric in the arrangement of their parts. It is more surprising that they should be so distinct in their action as they are. In reference to bismuth, and many other bodies, it is probable that magnetic force will

give a more important indication in relation to the essential and real crystalline structure of the mass than its form can do.

2479. I have already stated that the *magnecrystallic* force does not manifest itself by attraction or repulsion, or, at least, does not cause approach or recession, but gives *position* only. The *law* of action appears to be, that, *the line or axis* of MAGNECRYSTALLIC *force* (being the resultant of the action of all the molecules), *tends to place itself parallel, or as a tangent, to the magnetic curve or line of magnetic force, passing through the place where the crystal is situated.*

2480. I now broke up masses of bismuth which had been melted and solidified in the ordinary way, and, selecting those fragments which appeared to be most regularly crystallized, submitted them to experiment. It was almost impossible to take a small piece which did not obey the magnet and point more or less readily. By selecting the thin plates with perfect cleavage planes, I readily obtained specimens which corresponded in all respects with the crystals; but thicker plates or angular pieces often proved complicated in the results, though apparently simple and regular as to form. Occasionally, the cleavage plane, which I have beforehand taken for that perpendicular to the line of force (2475.), has proved not to be the plane supposed; but, after observing experimentally the direction of the magnecrystallic power, I have always either found, or else obtained by cleavage, a plane corresponding to it, possessing the appearance and character before described (2475.). Bismuth plates from the one-twentieth to the one-tenth of an inch in thickness, and bounded by parallel and similar planes, when broken up often proved, upon ocular examination, to be compounded and irregular.

2481. When a well-selected plate of bismuth (mine are about 0·3 of an inch in length and breadth, and 0·05, more or less, in thickness) is hung up by the edge in the magnetic field, it vibrates and points, presenting its faces to the magnetic poles, and setting diametrally (2461.). By whatever part of the edge it is suspended, the same results follow. But if it be suspended horizontally, the cleavage planes of the fragment and of the magnetic axis being parallel to the plane of motion of the plate, then it is perfectly indifferent; for then the line of magnecrystallic force is perpendicular to the line of magnetic force in every position that it can take.

2482. But if the plate be inclined only a very small quantity from this position, it points, and that with more force as the planes become more nearly vertical (2475.); and the phenomena before described with a crystal (2476.), can here be obtained with a fragment from a mass, and any part of the edge of the plate made to point axially, by elevating or depressing it above or below the horizontal plane.

2483. If a number of these crystalline plates be selected at the magnet, they may afterwards be built up together, with a little good cement (2458.), into a mass which has perfectly regular magnecrystallic action; and in that respect resembles the

crystals before spoken of (2459. 2468. 2476.). In this manner, also, the *diamagnetic* effect of the bismuth may be neutralized; for it is easy to build up a prism whose breadth and thickness is equal, and this being hung with the length vertical, points well and without any interference of diamagnetic action.

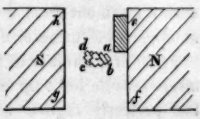
2484. By placing three equal plates at right angles to each other, a system is obtained, which has lost all power of pointing under the influence of the magnet, the force being, in every direction, neutralized. This represents the case of finely crystallized or amorphous bismuth. The same result (having the same nature) may be obtained by taking a selected uniform mass of crystals (2457.), melting it in a glass tube and resolidifying it: unless the crystallization is large and distinct, which rarely happens, the piece obtained is apparently without magnecrystallic force. A like result is also obtained by breaking up the crystal and putting the small fragments or powder into a tube, and submitting the whole to the force of the magnet.

2485. These experiments on bismuth are not difficult of repetition; for, except those which require the sudden production or cessation of the magnetic force, the whole may be repeated with an ordinary horse-shoe magnet. A magnet, with which I have wrought considerably, consists of seven bars placed side by side, and being fixed in a box with the poles upwards, presents two magnet cheeks, an inch and a quarter apart, between which is the magnetic field, having the lines of force in a horizontal direction. The poles of the magnet should be covered, each with paper, to prevent communication of particles of iron or rust. The best place for the piece of bismuth is, of course, between the poles; not level however with their tops, but from 0·4 to 1·0 inch lower down (2463.), that the effect of flat-faced poles may be obtained. If it be desired to strengthen the lines of magnetic force, this may be done by introducing a piece of iron between the poles of the magnet, and so, by virtually causing them to approach, lessen the width of the magnetic field between them.

2486. The magnet I used would sustain 30 lbs. at the keeper; but employing small pieces of bismuth, I have easily obtained the effects with magnets weighing themselves not more than 7 ounces, and able to sustain only 22 ounces; so that the experiments are within the reach of every one.

2487. Whilst the crystal of bismuth is in the magnetic field, it is affected very distinctly, and even strongly, by the near approximation of soft iron or magnets, and after the following manner. Let fig. 1 represent in plan the position of the two chief magnetic poles, and of a piece of crystallized bismuth between them, which, by its magnecrystallic condition, points axially. Then, if a piece of soft iron be applied against the cheek of the pole, as at *e*, and also near to the bismuth, as at *a*, it will affect the latter and

Fig. 1.

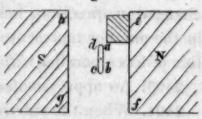


cause its approach to the iron. If the iron be applied in a similar manner at *f*, *g*, or *h*, it will have a like result in causing motion of the bismuth; and the parts marked *b*, *c* and *d*, will in turn approach it, seeming to be attracted. If the soft iron do not touch the magnetic pole, but be held between it and the bismuth so as to represent generally the same positions, the same effects, but in a weaker degree, are produced.

2488. Though these motions seem to indicate an effect of attraction, I do not believe them to be due to any such cause, but simply to the influence of the law of action (2479.) before expressed. The previously uniform condition of the magnetic field is destroyed by the presence of the iron; lines of magnetic force, of greater intensity than the others, proceed from the angle *a* of the iron in the position represented, or from the corresponding angles in the other positions (the shape of the pole now approximating more or less to the conical or pointed form), and therefore the crystal of bismuth moves round on the axis of suspension, that it may place the line of magnecrystallic force parallel or as a tangent to the resultant of the magnetic forces which pass through its mass.

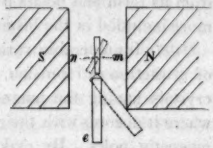
2489. When in place of the group of crystals a crystalline plate of bismuth (2481.) is employed, the appearances produced under similar circumstances, are those of *repulsion*; for if fig. 2 be allowed to represent this state of things, the piece of iron applied at *e* causes the plate to recede from it at *a*, or if applied at *f*, *g*, or *h*, it causes recession of the bismuth from it at the points *b*, *c*, and *d*. Now though these effects look like repulsion, they are, as I conclude, nothing more than the consequences of the endeavour which the bismuth makes, under the law before expressed (2479.), to place the magnecrystallic line of force parallel to, or as a tangent to the resultant of magnetic force passing through the bismuth.

Fig. 2.



2490. A piece of iron wire about 1½ inch long, and 0·1 or 0·2 of an inch thick, being held in the equatorial plane to the edge of the plate (fig. 3), did not alter its position; but if the end *e* were inclined to either pole, the plate began to move, and moved most when the iron touched the pole as in the figure. When it approached or touched the N pole, the inclination of the crystal plate of bismuth was as indicated by the dotted figure. When it touched the S, the inclination was the contrary way. If the end *e* were kept in contact with the N pole, and the other end of the soft iron rod placed in the position *m*, the bismuth was not affected; but if then this subsidiary pole were moved the one way or the other towards the edge of the plate, the latter turned as the pole moved, always tending to keep its face towards it, and evidently by the tendency of the magnecrystallic axis to place itself parallel to the resultant of magnetic force

Fig. 3.



passing through the bismuth. The same results were obtained with the crystal (2487.) under similar circumstances, and corresponding results were obtained when the soft iron rod was applied between the S cheek of the magnet and the bismuth. The like effects were also obtained with plates of arsenic and antimony.

2491. When a magnet is used instead of soft iron, corresponding effects are produced; only, it must be remembered, that if the chief magnet be very powerful, it may often neutralize, and even change, the magnetism of the small approximated magnet; and this can happen with the latter (as to external influence) whilst in the magnetic field, even though when withdrawn it may appear to remain unaltered.

2492. Thus, when the plate of bismuth was suspended between the cheeks of the horse-shoe magnet (2485.), fig. 2, and the north pole of a small magnet (the blade of a pocket-knife) was placed at *a* or *b*, it caused recession of the part of the bismuth near it, and precisely for the same reasons as those that existed when the soft iron was there. When the extra pole was placed at *c* or *d*, the action was more feeble than in the former case, and consisted in an approximation of that part of the bismuth to the pole. As this position of the subordinate pole would terminate and neutralize certain of the lines of magnetic force proceeding from the south pole of the horse-shoe magnet, so the resultant of the lines of force passing through the bismuth would be changed in direction, being rendered oblique to their former course, and precisely in the manner represented by the motion of the bismuth, in its tendency to place its line of force parallel with them in their new position.

2493. An approximated south pole caused motions in the contrary direction.

2494. When the subordinate pole was applied to the edge of the plate, the little magnet being in the equatorial position (fig. 3), then instead of being neutral, as the iron was, it caused the plate to move in a tangential direction, either to the right or the left, according as it was either a south or a north pole, just indeed as the iron did when, by inclining it, the approximated end became a pole (2490.). This effect was shown in a still more striking degree by using the crystal of bismuth (2487.), because, from its form and position the magnetic curves most affected by the extra pole were more included in the bismuth than when the plate was used.

2495. Innumerable variations of these motions may be caused, and appearances of attraction or repulsion, or tangential action be obtained at pleasure by the use of crystals having the magnecrystallic axis corresponding with their length, or plates where it accords with their thickness; and either permanent or temporary subsidiary magnetic poles. By making the moveable pole travel slowly round the bismuth from the neutral point *m* to the other neutral point *n*, fig. 3, a summary of the whole can be obtained, and it is found that they all resolve themselves into the general law before expressed (2479.): the magnecrystallic axis and the resultant of magnetic force passing through the bismuth, tending to become parallel.

2496. Hence a small crystal or plate of bismuth (or arsenic (2532.)) may become

a very useful and important indicator of the direction of the lines of force in a magnetic field, for at the same time that it takes up a position showing their course, it does not by its own action tend sensibly to disturb them.

2497. Many of these motions are similar to, and have relation with, those described by PLÜCKER, REICH, and others, as obtained by the action of iron and magnets on bismuth, in its simple diamagnetic condition. These results are by them and others considered as indicating that the bismuth, as I had originally supposed (2429, &c.), has really in its diamagnetic state, a magnetic condition the reverse of that of iron. I am not acquainted with all of them, or with the reasoning thereon (being in the German language); but such as I am aware of, and have reobtained, seem to me to be simple results of the law I formerly laid down (2267. 2418.), namely, that diamagnetic bodies tend to proceed from stronger to weaker places of magnetic force: and give no additional or other proof of the assumed reverse polarity of bismuth than the former cases of action which I had given, coming under that law.

2498. Supposing that the intervening or surrounding matter might, in some manner, affect the magnecrystallic action of bismuth and other bodies, I fixed the magnetic poles at a given distance (about two inches) asunder, suspended a crystal of bismuth in the middle of the magnetic field, and observed its vibrations and set. Then, without any other change, I introduced screens of bismuth, being blocks about two inches square and 0·75 of an inch in thickness, between the poles and the crystal, but I could not perceive that any change in the phenomena was produced by their presence.

2499. The bismuth crystal (2459.) was suspended in water between the magnetic poles of the horse-shoe magnet. It set well in accordance with the general law (2479.), and it took five revolutions of the torsion index at the upper end of the suspending silk filament to displace it, and cause it to turn into the diametral position. This is, as well as I could observe the results, the same amount of torsion force required to effect its displacement when the crystal was placed in the same position, but surrounded with air only.

2500. The same bismuth was then suspended in a saturated solution of protosulphate of iron (adapted as a magnetic medium), it set as before with apparently no change of any kind; and when the torsion force was put on, it still required five turns of the index, as before, to cause the displacement of the crystal, and its passage into the diametral position.

2501. Whether therefore crystals of bismuth be immersed in air, or water, or solution of sulphate of iron, or placed between thick masses of bismuth, if they be subject to the same magnetic force, the magnecrystallic force exerted by them is the same both in nature, direction and amount.

2502. It seemed possible and probable that magnetic force might affect the crystallization of bismuth, if not of other bodies. For, as the force affects the mass of a crystal by that power which its particles possess, and which they give to the crystal as a whole by their polar (or axial (2472.)) and symmetric condition; and, as the final position of the crystalline mass in the magnetic field may be considered as that of the least constraint, so it was likely enough that, if the bismuth in a fluid state were placed under the influence of the magnetism, the individual particles would tend to assume one and the same axial condition, and the crystalline arrangement and direction of the mass upon its solidification, be in some degree determined and under government.

2503. Some bismuth, therefore, was fuzed in a glass tube and held in a fixed position in the strong magnetic field until it had become solid; then, being removed from the glass, it was suspended so that it might assume the same position under the influence of the magnet; but no signs of *magnecrystallic* force were evident. It was not expected that the whole would become regularly crystallized, but that a *difference* between one direction and another might appear. Nothing of the kind however occurred, whatever the direction in which the piece was suspended; and when it was broken open, the crystallization within was found to be small, confused, and in all directions. Perhaps if longer time were allowed, and a permanent magnet used, a better result might be obtained. I had built many hopes upon the process, in reference to the crystalline condition of gold, silver, platina, and the metals generally, and also in respect of other bodies.

2504. I cannot find that crystals of bismuth acquire any power, either temporary or permanent, which they can *bring away* from the magnetic field. I held crystals in different positions in the field of intense action of a powerful electromagnet, having conical terminations very near to each other; and, after some time, removed them and applied them instantly to a very delicate astatic magnetic needle; but I could not perceive that they had the least extra effect upon it, because of such treatment.

2505. As a crystal of bismuth is subject to, and obeys the influence of, the lines of magnetic force (2479.), so it follows that it ought to obey even the earth's action, and point, though with a very feeble degree of power. I have suspended a good crystal by a single long filament of cocoon silk, and sheltered it as well as I could from currents of air by concentric glass tubes, and I think have observed indications of a set or pointing. The crystal was so hung that the magnecrystallic axis made the same angle with the horizontal plane (about 70°) as the magnetic dip, and the indication was, that the axis and the dip tended to coincide: but the experiments require careful repetition.

2506. A more important point, as to the nature of the polar or axial forces of bis-

muth, is to know whether two crystals, or uniformly crystallized masses of bismuth, can mutually affect each other; and if so, what the nature of these affections are? what is the relation of the equatorial and terminal parts? and what, the direction of the forces? I have made many experiments, in relation to this subject, both in and out of the magnetic field, but obtained only negative results. I employed however small masses of bismuth, and it is my purpose to repeat and extend them at a more convenient season with larger masses, built up, if necessary, in the manner already described (2483.).

2507. I need hardly say that a crystal of bismuth ought to point in a helix or ring of wire carrying an electric current, and so that its magnecrystrallic axis should be parallel to the axis of the ring or helix. This I find experimentally to be the case.

¶ ii. *Crystalline Polarity of Antimony.*

2508. *Antimony* is a *magnecrystallic* body. Some crystalline masses, procured in the manner before described (2457.), were broken up with copper tools, and some excellent groups of crystals were obtained, weighing from ten to twenty grains each, in which all the constituent crystals appeared to be uniformly placed. The individual crystals were very good on the whole, and much more frequently full at the faces and complete than those of bismuth. They were very bright, having a steel-gray or silvery appearance, and to the eye appeared more surely as cubes than bismuth, though here and there distinctly rhomboidal faces presented themselves. Planes of cleavage can be made to replace the solid angles; and, as with bismuth, there is one plane generally brighter and more perfect than the others.

2509. In the first place, it was ascertained that all these crystals were diamagnetic, and strongly so.

2510. In the next it was ascertained, as with bismuth, that all of them exhibited the magnecrystallic phenomena with considerable power, showing the existence of a line of force (2470.), which, when placed vertically, left the crystal free to move in any direction (2476.); but when placed horizontally, caused the crystal to point, and in so doing took up its own position parallel to the resultant of magnetic force passing through the crystal (2479.). This line proceeded, as in bismuth, from one of the solid angles to the opposite one, and was perpendicular to the bright cleavage plane just spoken of (2508.).

2511. So, generally, the action of the magnet upon these crystals was the same as upon the crystals of bismuth; but there are some points of variation which require to be more distinctly stated and distinguished.

2512. In the first place, when the magnecrystallic axis was horizontal, and a certain crystal used, upon the evolution of the magnetic force, the crystal went up to its position slowly, and pointed as with a dead set. If the crystal were moved from this position on either side, it returned to it at once: there was no vibration. Other crystals did the same imperfectly; and others again made one or perhaps two vibra-

tions, but all appeared as if they were moving in a thick fluid, and were, in that respect, utterly unlike bismuth, in the freedom and mobility with which it vibrated (2459.).

2513. In the next place, when the crystals were so suspended as to have the magnecrystallic axis vertical, there was no pointing nor any other signs of magnecrystallic force; but other appearances presented themselves. For, if the crystalline mass was revolving when the magnetic force was excited, it suddenly stopped, and was caught in a position which might, as was found by experience, be any position; but if the greatest length was out of the axial or equatorial position, the arrest was followed by a *revulsive* motion on the discontinuance of the electric current (2315.). This revulsive motion was never great, but was most when the length of the mass formed about an angle of 45° with the axis of the magnetic field.

2514. On further examination it appeared that this arresting and revulsive effect was precisely the same in kind as that observed on a former occasion with copper and other metals (2309.), and due to the same cause, namely, the production of circular electric currents in the metal under the inductive force of the magnet. Now, the reason appeared why, in the former case, the crystals of antimony did not oscillate (2512.); and why, also, they went up to their position of rest with a dead set; for the currents produced by the motion are just those which tend to stop the motion (2329.)*; and though the magnecrystallic force was sufficient to make the crystal move and point, yet the very motion so produced generated the current which reacted upon the tendency to motion, and so caused the mass to advance towards its position of rest as if it moved in a thick fluid.

2515. Having this additional knowledge respecting the arrest and revulsion of the antimony (effects dependent upon its superior conducting power, in this compact crystalline state, as compared with bismuth), one has no difficulty in identifying the magnecrystallic force of this metal with that of the former, and the correspondence of the results in all essential characters and particulars. In most of the pieces of crystals of antimony the force seemed less than in bismuth, but the fact may not really be so, for the inductive current action just described, tends to hide the magnecrystallic phenomena.

2516. Different pieces of antimony also seem to differ from each other in their setting force, and also in their tendency to exhibit revulsive effects; but these differences are either only apparent, or may easily be explained. The arresting and revulsive action depends much upon the continuity of the mass, so that one large piece shows it much better than several small pieces, and these again better than a

* Any one who wishes to form a sufficient idea of the arresting powers of these induced currents, should take a lump of solid copper, approaching to the cubical or globular form, weighing from a quarter to half a pound; should suspend it by a long thread, give it a rapid rotation, and then introduce it, spinning, into the magnetic field of the electro-magnet; he will find its motion to be instantly stopped; and if he further tries to spin it, whilst in the field, will find it impossible to do so.

powdered substance. Even the revulsive action of copper may be entirely destroyed by reducing the single lump to filings. It is easy to perceive, therefore, that of two groups of antimony crystals, each symmetrically disposed within itself, the one may have larger crystals well connected together, as regards the induction of currents through the whole mass, and the other smaller crystals less favourably united. These would present very different appearances, as regards the arrest of motion and succeeding revulsive action; and further, on that very account, would differ in their readiness to present the magnecrystallic phenomena, though they might possess precisely equal degrees of that force.

2517. On proceeding to experiment with plates of antimony, further illustrations of the effects resulting from the causes just described were obtained, with abundant accompanying evidence of the existence of the magnecrystallic condition in the metal. The plates were selected from broken masses, as with bismuth (2480.). Some were soon found which acted simply, instantly, and well; their large surfaces were bright cleavage planes. When suspended by any part of the edge, these planes faced towards the magnetic poles; and the plate oscillated on each side of its final position, gradually acquiring its state of rest.

2518. When these plates were suspended with their planes horizontal, they had no power of pointing in the magnetic field. When they were inclined, the points which were most depressed below and raised above the horizontal plane, were those which took up their places nearest the magnetic poles (2482.).

2519. When several plates were arranged together into a consistent bundle (2483.), the *diamagnetic* effect was removed, and the magnecrystallic oscillation and pointing became very ready and characteristic.

2520. Thus it is evident that, in all these cases, there was a line of magnecrystallic force perpendicular to the planes of the plates, and perfectly consistent in its position and action with the force before found in the solid crystals of antimony.

2521. But another plate of antimony was now selected, which had every appearance of being able to present all the phenomena of the former plates; and yet, when hung up by its edge, it showed no signs of magnecrystallic results; for it first advanced a little (2310.), then was arrested and kept in its place, and if standing between the axial and equatorial positions, was revulsed when the battery current was interrupted, exhibiting effects equal to those of copper (2315.). Many other plates were tried with precisely the same result.

2522. When this plate (2521.) was placed in the field of intense power between two *conical* magnetic poles, it exhibited the same phenomena; but notwithstanding the arresting action, it moved slowly until it stood in the equatorial position; a result which was probably due to the exertion of both *magnecrystallic* and *diamagnetic* force. When the plate was suspended with its planes horizontal, the arresting and revulsive actions were gone; for the induced currents which before caused them could not now exist in the necessary vertical plane; further, it had no setting power,

which showed that there was no axis of magnecrystallic force in the length or breadth of the plates.

2523. Other plates were then found able to produce mixed effects, and those in different degrees. Thus, some, like the first, vibrated freely, pointed well, and presented no indication of the arrest and revulsive phenomena. Others vibrated sluggishly, set well, and showed a tendency to be arrested. Others pointed well, going up to their place with a dead set, but moving as if in a fluid; or, if the magnetic force were taken off before the piece had settled, it was revulsed feebly: and others were caught at once, did not set (within the time of my observation), and were strongly revulsed.

2524. Finally, a careful investigation, carried on by means both of the horse-shoe (2485.) and the great electro-magnet (2247.), made the cause of these differences in the effects apparent.

2525. It may be observed, in the first place, that sometimes a plate of antimony being selected (2517.), having planes very bright and perfect in their appearance, and, therefore, giving reason to think that it may point well in the magnetic field, when submitted to the horse-shoe magnet does not do so; but points obliquely, feebly, and perhaps in two undiametral positions. This is, I have no doubt, because the crystallization is complicated and confused. Such a plate, if it be sufficiently broad and long (i. e. not less than a quarter or one-third of an inch), when submitted to the electro-magnet, will show the arresting (2310.) and revulsive (2315.) action well.

2526. In the next place, we have to remember that, for the development of the induced currents that cause the arresting and revulsive action, the plate must have certain sufficient dimensions in a vertical plane (2329.). The currents occur in the mass and not round the separate particles (2329.), and the resultant of the magnetic lines of force passing through the substance, is the axis round which these currents are produced. Hence the reason why the effect does not occur with plates suspended in the horizontal position, which yet produce it well in the vertical position; a result which a disc half an inch in diameter of thin foil or plate, being copper, silver, gold, tin, or almost any malleable metal will show; though the best conductors are the fittest for the purpose. Now this condition is of no consequence in respect of *magnecrystallic* action, and a narrow plate has as much force as a broad one, having the same mass. The first plate that I happened to select (2517.) was well crystallized, thick and narrow; hence it was favourable for magnecrystallic action, unfavourable to the arresting and revulsive action, and showed no signs, comparatively, of the latter.

2527. When a broad and well-crystallized plate is obtained, then both sets of effects appear: thus, if the plate is revolving when the magnetic force is brought into action, it quickens its velocity for an instant, then is stopped; and if the magnetic force is at once taken off, it is revulsed, exactly as a piece of copper would be

(2315.). But if the magnetic force be continued, it will then be perceived that the stop is only apparent; for the plate moves, though with a greatly reduced velocity, and continues to move until it has taken up its magnecrystallic position. It moves as if in a thick fluid. Hence the magnecrystallic force is there and produces its full effect; and the reason why the appearances have changed is, that the very motion which the force tends to give, and does give to the mass, causes those magneto-electric currents (2329.) which by their mutual action with the magnet tends to stop the motion; and therefore its slowness and the final dead set (2512. 2523.).

2528. A magnet which is weaker (as the horse-shoe instrument described (2485.)) produces the currents by induction in a much weaker degree, and yet manifests the magnecrystallic power well; hence it is more favourable, under certain circumstances, for such investigations; as it helps to distinguish the one effect from the other.

2529. It will readily be seen that plates, whether of the same metal or of different metals, cannot, even roughly, be compared with each other as to magnecrystallic force by their vibrations; for under the influence of these induced currents, plates of the same magnecrystallic force oscillate in very different manners. I took a plate, and by cement (2458.) attached selected paper to its faces, and then observed how it acted in the magnetic field; it set slowly, and it showed the arresting and revulsive effects (2521.). I then pressed it in a mortar, so as to break it up into many parts, which still kept their place; and now it set more freely and quickly, and showed very little of the revulsing action.

2530. Though the indication by vibration is thus uncertain, the torsion force still remains to us, I believe, a very accurate indication of the strength of the set (2500.); and, therefore, of the degree of the magnecrystallic force; and though the suspending silk fibre may give way a little, a glass thread, according to RITCHIE'S suggestion, would answer perfectly.

2531. Antimony must be a good conductor of electricity in the direction of the plates of the crystals, or it would not give, so freely, these indications of revulsive action. The groups of crystals of antimony (2508.) showed the effect in such a degree, as to make me think that the constituent cubes possessed the power nearly equally in all directions. A piece of finely crystallized or granular antimony does not, however, show it in the same proportion; from which it would seem as if an effect equivalent in some degree to that of division occurs, either at the meeting of two incongruous crystals, or between the contiguous plates of the crystals, and affects the conducting power in these directions.

¶ iii. *Crystalline Polarity of Arsenic.*

2532. A mass of the metal arsenic exhibiting crystalline structure (2480.), was broken up, and several plates selected from the fragments, having good cleavage plane surfaces, about 0·3 of an inch in length, 0·1 inch in width, and 0·03 in thick-

ness. These, when suspended opposite one conical pole, proved to be perfectly diamagnetic; and when before it or between two poles strongly *magnecrystallic*. I have a pair of flat-faced poles with screw-holes in the centre of the faces, and these so much weaken the intensity of the lines of magnetic force about the middle of the field, when the faces are within half an inch of each other, that a cylinder of granular bismuth 0·3 in length sets *axially*, or from pole to pole (2384.). But with the plates of arsenic between the same poles there was no tendency of this kind; so much was the magnecrystallic force predominant over the diamagnetic force of the substance.

2533. When the plates of arsenic were suspended with their planes horizontal, then they did not point at all between the flat-faced poles. Any inclination of the planes to the horizontal line produced pointing, with more or less force as the planes approached more or less to the vertical position, exactly in the manner already described in relation to bismuth and antimony (2482. 2518.).

2534. Thus, arsenic with bismuth and antimony are found to possess the magnecrystallic force or condition.

Royal Institution,
September 23, 1848.

II. *Experimental Researches in Electricity.—Twenty-second Series (continued).*

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§ 28. *On the crystalline polarity of bismuth and other bodies, and on its relation to the magnetic and electric form of force (continued).*

¶ iv. *Crystalline condition of various bodies.* ¶ v. *Nature of the magnecrystallic force, and general observations.*

¶ iv. *Crystalline condition of various bodies.*

2535. *ZINC.*—Plates of zinc broken out of crystallized masses gave irregular indications, and, being magnetic from the impurity in them, the effects might be due entirely to that circumstance. Pure zinc was thrown down electro-chemically on platina from solutions of the chloride and the sulphate. The former occurred in ramifying dendritic associations of small crystal; the latter in a compact close form. Both were free from magnetic action and freely diamagnetic, but neither showed any trace of the magnecrystallic action.

2536. *Titanium* *.—Some good crystals of titanium obtained from the bottom of an iron furnace, were cleansed by the alternate action of acids and fluxes until as clear from iron as I could procure them. They were bright, well-formed and magnetic (2371), and contained iron, I think, diffused through their whole mass, for nitro-muriatic acid, by long boiling, continually removed titanium and iron from them. These crystals had a certain magnetic property which I am inclined to refer to their crystalline condition. When between the poles of the electro-magnet, they set; and when the electric current was discontinued, they still set between the poles of the enfeebled magnet as they did before. If left to itself a crystal always took the *same* position, showing that it was constantly rendered magnetic in the same direction. But if a crystal was placed and kept in another position between the magnetic poles whilst the electric current was on, and afterwards the current suspended, and then the crystal set free, it pointed between the poles of the enfeebled magnet in this new direction; showing that the magnetism was in a different direction in the body

* For these and many other crystals I am indebted to the kindness of Sir HENRY T. DE LA BECHE and Mr. TENNANT.

of the crystal to that which it had before. If now the magnet were reinvigorated by the electric current, the crystal instantly spun round and took a magnetic state in the first or original direction. The crystals could in fact become magnetized in any direction, but there was one direction in which they could be magnetized with a facility and force greater than in any other. From the appearances I am inclined to refer this to the crystalline condition, but it may be due to an irregular diffusion of iron in the masses of titanium. The crystals were too small for me to make out the point clearly.

2537. *Copper.*—I selected some good crystals of native copper, and, having carefully separated them from the mass, examined them in respect of their magnecrystallic force. At the horse-shoe magnet (2486.) they gave no signs of such power, whatever the direction in which they were suspended, but stood in any position; and any degree of torsion, however small, applied at the upper extremity of the suspending filament was obeyed at once, and to the full extent, by the crystal beneath. When subjected to the electro-magnet, the phenomena of arrest and revulsion were produced (2513. 2310.), as was to be expected. If after the arrest the magnetic force were continued, there was no slow advance of the crystal up to a distinct pointing position (2512.); it stood perfectly still in any position. So there is no evidence of magnecrystallic action in this case.

2538. *Tin.*—I selected from block and grain tin some pieces which appeared, by their external forms and the surface produced under the action of acids, to have a regular crystalline structure internally; and, cutting off portions, carefully submitted them to the power of the magnets, but there was no appearance of any magnecrystallic phenomena. Indications of the arresting and revulsive actions were presented, and also of diamagnetic force, but nothing else. I also examined some crystals of tin obtained by electro-chemical deposition. They were pure and diamagnetic: they were arrested and revulsed, but they showed no signs of magnecrystallic action.

2539. *Lead.*—Lead was crystallized by fusion, partial solidification, and pouring off (2457.), and some very fair crystals, having the general form of octohedra, obtained. Observed at the magnets, these were arrested and revulsed feebly, but presented no magnecrystallic phenomena. Some fine crystalline plates of lead obtained electro-chemically from the decomposition of the acetate by zinc, were submitted to the magnet: they were pure, diamagnetic, and were arrested and revulsed, but presented no appearance of magnecrystallic action.

2540. *Gold.*—Three fine large crystals of gold were examined. They were diamagnetic, and easily arrested (2310. 2340.); the revulsion did not take place, because of their octohedral or orbicular form. They presented no magnecrystallic indications.

2541. *Tellurium.*—Two fractured pieces of this substance, presenting large and parallel planes of cleavage, were examined: both pointed, and the greatest length was across the axial line between flat-faced poles (2463.). I think the effects were

in part, if not altogether, due to the magnecrystallic state of the substance; but I do not think the evidence was quite conclusive.

2542. *Iridium and Osmium alloy.*—The native grains of iridium and osmium are often flat, presenting two planes looking like crystal planes, which are parallel to each other even when the grains are thick. Some of the largest and most crystalline were selected, and, after ignition with flux and digestion in nitromuriatic acid, were examined at the magnet. Some were more magnetic than others, being attracted; others were very little magnetic: the latter were selected and examined more carefully. These all pointed with great readiness and force, comparatively speaking; for they were not above one-fifteenth of an inch long, and yet they set freely when the magnetic poles were three or four inches apart. The faces of the crystalline particles were *always* towards the poles, and their length consequently not *in* but *across* the axial line; and this was true whether the distance between the poles was small or great, or whether flat-faced or conical poles were used. I believe they were magnecrystallic.

2543. *Fusible metal.*—Crystals of fusible metal (2457.) pointed, but the crystals, which were apparently quadrangular plates or prisms, were not good, and the evidence not clear and distinct.

2544. *Wires.*—I thought it possible that thin wires, which by the action of acids exhibited fibrous arrangements, might have their particles in a state approaching to the crystalline condition, and therefore submitted bundles of platinum, copper, and tin wire to the action of the magnet; but no indications of magnecrystallic action appeared.

2545. I submitted several metallic compounds to the power of the magnet, applied so as to develope any indication of the magnecrystallic phenomena. Galena, native cinnabar, oxide of tin, sulphuret of tin, native red oxide of copper, Brookite or oxide of titanium, iron pyrites, and also diamond, fluor spar, rock-salt and boracite, being all well-crystallized and diamagnetic, presented no evidence of the magnecrystallic force. Native and well-crystallized sulphuret of copper, sulphuret of zinc, cobalt glance and leucites were magnetic. Arsenical iron, specular iron and magnetic oxide of iron were still more so. I could not in any of them distinguish any magnetic results due to crystallization.

2546. On examining magnetic salts, several of them presented very striking magnecrystallic phenomena. Thus, with *sulphate of iron*, the first crystal which I employed was suspended with the magnecrystallic axis vertical, and it presented no particular appearances; only the longest horizontal direction went into the magnetic axis pointing feebly. But on turning the piece 90° (2470.), instantly it pointed with much force, and the greatest length went equatorially. The crystal was compounded of superposed flat crystals or plates, and the magnecrystallic axis went directly across these; it was easy therefore, after one or two experiments, to tell beforehand how the crystal should be suspended, and how it would point. Whether the crystals were

long, or oblique, or irregular, still the magnecrystallic force predominated and determined the position of the crystal, and this happened whether pointed or flat poles were used, and whether they were near together or far asunder. The magnecrystallic axis is perpendicular, or nearly so, to two of the sides of the rhomboidal prism. I have some small prismatic crystals of which the length is nearly three times the width of the prism; but when both the length and the magnecrystallic axis are horizontal, no power of the magnet, or shape, or position of the poles, will cause the length to take the axial direction, for that is constantly retained by the magnecrystallic axis, so greatly does it predominate in power over the mere magnetic force of the crystal. Yet this latter is so great as at times to pull the suspending fibre asunder when the crystal is above the poles (2615.).

2547. *Sulphate of nickel.*—When a crystal of sulphate of nickel was suspended in the magnetic field, its length set axially. This might be due, either to mere magnetic force, or partly to magnecrystallic force. Therefore I cut a cube out of the crystal, two faces of which were perpendicular to the length of the original prism. This cube pointed well in the magnetic field, and the line coincident with the axis of the prism was that which pointed axially, and represented the magnecrystallic axis. Even when the cube was reduced in this direction and converted into a square plate whose thickness coincided with the magnecrystallic axis, it pointed as well as before, though the shortest dimensions of the piece was now axial.

2548. The *persulphate of ammonia and iron*, and the *sulphate of manganese*, did not give any indication of magnecrystallic phenomena; the *sulphate of ammonia and manganese* I think did, but the crystals were not good. The sulphate of potassa and nickel is magnecrystallic. All three salts were magnetic.

2549. Thus it seems that other bodies besides bismuth, antimony and arsenic, present magnecrystallic effects. Amongst these are the alloy of iridium and osmium, probably tellurium and titanium, and certainly the sulphates of iron and nickel. Before leaving this part of the subject, I may remark that this property has probably led me into error at times on a former occasion (2290.). A mistake with arsenic (2383.) might very easily arise from this cause.

¶ v. *On the nature of the magnecrystallic force, and general observations.*

2550. The magnecrystallic force appears to be very clearly distinguished from either the magnetic or diamagnetic forces, in that it causes neither approach nor recession; consisting not in attraction or repulsion, but in its giving a certain determinate position to the mass under its influence, so that a given line in relation to the mass is brought by it into a given relation with the direction of the external magnetic power.

2551. I thought it right very carefully to examine and prove the conclusion, that there was no connection of the force with either attractive or repulsive influences. For this purpose I constructed a torsion-balance, with a bifilar suspension of cocoon

silk, consisting of two bundles of seven filaments each, four inches long and one-twelfth of an inch apart; and suspended a crystal of bismuth (2457.) from one end of the lever, so that it might be fixed and retained in any position. This balance was protected by a glass case, outside of which the conical terminal of one pole of the great electro-magnet (2247.) was adjusted, so as to be horizontal, at right angles to the lever of the torsion-balance, and in such a position that the bismuth crystal was in the prolongation of the axis of the pole, and about half an inch from its extremity when all was at rest. The other pole, four inches off, was left large so that the lines of magnetic force should diverge, as it were, and rapidly diminish in strength from the end of the conical pole. The object was to observe the degree of repulsion exerted by the magnet on the bismuth, as a diamagnetic body, either by the distance to which it was repelled, or by the torsion required to bring it back to its first position; and to do this with the bismuth, having its magnecrystallic axis at one time axial or parallel to the lines of magnetic force, at another equatorial, observing whether any difference was produced.

2552. The crystal was therefore placed with its magnecrystallic axis first parallel to the lines of magnetic force, and then turned four times in succession 90° in a horizontal plane, so as to observe it under all positions of the magnecrystallic axis; but in no case could any difference in the amount of the repulsion be observed. In other experiments the axis was placed oblique, but still with the same result. If there be therefore any difference it must be exceedingly small.

2553. A corresponding experiment was made, hanging the crystal as a pendulum by a bifilar suspension of cocoon silk thirty feet in length, with the same result.

2554. Another very striking series of proofs that the effect is not due to attraction or repulsion, was obtained in the following manner. A skein of fifteen filaments of cocoon silk, about fourteen inches long, was made fast above, and then a weight of an ounce or more hung to the lower end; the middle of this skein was about the middle of the magnetic field of the electro-magnet, and the square weight below rested against the side of a block of wood, so as to give a steady, silken, vertical axis, without swing or revolution. A small strip of card, about half an inch long, and the tenth of an inch broad, was fastened across the middle of this axis by cement; and then a small prismatic crystal of sulphate of iron about 0·3 of an inch long, and 0·1 in thickness, was attached to the card, so that the length, and also the magnecrystallic axis, were in the horizontal plane; all the length was on one side of the silken axis, so that as the crystal swung round, the length was radius to the circle described, and the magnecrystallic axis parallel to the tangent.

2555. This crystal took a position of rest due to the torsion force of the suspending skein of silk; and the position could be made any one that was desired, by turning the weight below. The torsion force was such, that, when the crystal was made to vibrate on its silken axis, forty complete (or to and fro) vibrations were performed in a minute.

2556. When the crystal was made to stand between the flat-faced poles (2463.) obliquely, as in fig. 4, the moment the magnet was excited it moved, tending to stand with its length equatorial or its magnecrystallic axis parallel to the lines of magnetic force. When the N pole was removed, and the experiment repeated, the same effect took place, but not as strongly as before; and when, finally, the pole S was brought as near to the crystal as it could be, without touching it, the same result occurred, and with more strength than in the last case.

Fig. 4.

2557. In the two latter experiments, therefore, the crystal of sulphate of iron, though a magnetic body and strongly attracted by such a magnet as that used, actually *receded* from the pole of the magnet under the influence of the magnecrystallic condition.

2558. If the pole S be removed and that marked N be retained for action on the crystal, then the latter approaches the pole, urged by both the magnetic and magnecrystallic forces; but if the crystal be revolved 90° to the left, or 180° to the right, round the silken axis, so as to come into the contrary or opposite position, then this pole repels or rather causes the removal to a distance of the crystal, just as the former did. The experiment requires care, and I find that conical poles are not good; but with attention I could obtain the results with the utmost readiness.

2559. The sulphate of iron was then replaced by a crystalline plate (2480.) of bismuth, placed as before on one side of the silk suspender, and with its magnecrystallic axis horizontal. Making the position the same as that which the crystal had in relation to the N pole in the former experiment (2556.), so that to place its axis parallel to the lines of magnetic force it must approach this magnetic pole, and then throwing the magnet into an active state, the bismuth moved accordingly, and did approach the pole, against its diamagnetic tendency, but under the influence of the magnecrystallic force. The effect was small but distinct.

2560. Anticipating, for a short time, the result of the reasoning to be given further on (2607.), I will describe a corresponding effect obtained with the red ferro-prussiate of potassa. A crystal of this salt had its acute linear angles ground away, so as to convert it into a plate with faces parallel to the plane of the optic axis, and was then made to replace the plate of bismuth. Being in the position before represented (2556.), and the magnet rendered active, it moved, placing the plane of the optic axes equatorially, as PLÜCKER describes. When the pole N was removed and S brought up to the crystal, the same motion occurred, the crystal *retreating from* the pole; and when S pole was removed and N brought towards the crystal, it moved as before, the whole body now *approaching* towards the pole. On inclining the crystal the other way, i. e. making its place on the other side of the equatorial line, the S pole caused it to *approach* and the N pole to *recede*. So that the same pole seemed able either to attract or repel the same side of the crystal; and either pole could be made to show this apparent attractive and repulsive force.

2561. Hence a proof that neither attraction nor repulsion causes the set, or

governs the final position of the body, or of any of the bodies whose movements are due to the same cause (2607.).

2562. This force then is distinct in its character and effects from the magnetic and diamagnetic forms of force. On the other hand, it has a most manifest relation to the crystalline structure of the bismuth and other bodies; and therefore to the molecules, and to the power by which these molecules are able to build up the crystalline masses. It appears to me impossible to conceive of the results in any other way than by a mutual reaction of the magnetic force, and the force of the particles of the crystal on each other: and this leads the mind to another conclusion, namely, that as far as they can act on each other they partake of a like nature; and brings, I think, fresh help for the solution of that great problem in the philosophy of molecular forces, which assumes that they all have one common origin (2146.).

2563. Whether we consider a crystal or a particle of bismuth, its polarity has a very extraordinary character, as compared with the polarity of a particle in the ordinary magnetic state, or when compared with any other of the dual conditions of physical force; for the opposite poles have *like* characters; as is shown first of all by the diametral pointing of the masses (2461.), and also by the physical characters and relations of crystals generally. As the molecules lie in the mass of a crystal, therefore, they can in no way represent, or be represented by, the condition of a parcel of iron filings between the poles of a magnet, or the particles of iron in the keeper when in its place; for these have poles of *different* names and quality adhering together, and so giving a sort of structure; whereas, in the crystal, the molecules have poles of like nature towards each other, for, so to say, all the poles are alike.

2564. As made manifest by the phenomena, the magnecrystallic force is a force acting at a distance; for the crystal is moved by the magnet at a distance (2556. 2574.), and the crystal also can move the magnet at a distance. To produce the latter result, I converted a steel bodkin, about three inches long, into a magnet; and then suspended it perpendicularly by a single cocoon filament four inches long, from a small horizontal rod, which again was suspended by its centre and another length of cocoon filament, from a fixed point of support. In this manner the bodkin was free to move on its own axis, and could also describe a circle about $1\frac{1}{2}$ inch in diameter; and the latter motion was not hindered by any tendency of the needle to point under the earth's influence, because it could take any position in the circle and yet remain parallel to itself.

2565. A support perfectly free from magnetic action was constructed of glass rod and copper wire, which passing through the bottom of the stand, and being in the prolongation of the upper axis of motion, was concentric with the circle which the little magnet could describe; its height was such that it could sustain a crystal or any other substance level with the pole at the lower end of the needle, and in the centre of the small circle in which the latter could revolve around it. By moving the lower end of the support, the upper end also could be made to approach to or

recede from the magnet. The whole was covered with a glass shade, and when left to become of uniform temperature, and at rest, the needle magnet was found to take up a *constant* position under the torsion force of the suspending filaments. Further, any rotation of the glass and copper wire support did not produce a final change in the position of the magnet; for though the motion of the air would carry the magnet away, it returned, ultimately, to the same spot. When removed from this spot, the torsion force of the silk suspension made the system oscillate; the time of a half oscillation, or a passage in one direction, was about three minutes, and of a whole oscillation therefore six minutes.

2566. When a crystal bismuth was fixed on the support with the magnecrystallic axis in a horizontal direction, it could be placed near the lower pole of the magnet in any position, and being then left for two or three hours, or until by repeated examination the magnetic pole was found to be stationary, the place of the latter could be examined and the degree and direction in which it was affected by the bismuth ascertained. Extreme precaution was required in these observations, and all steel or iron things, as spectacles, knives, keys, &c., had to be dismissed from the observer before he entered the place of experiment; and glass candlesticks were used. The effect produced was but small, but the result was, that if the direction of the magnecrystallic axis made an angle of 10°, 20°, or 30° with the line from the magnetic pole to the middle of the bismuth crystal, then the pole followed it, tending to bring the two lines into parallelism; and this it did whichever end of the magnecrystallic axis was towards the pole, or whichever side it was inclined to. By moving the bismuth at successive times, the deviation of the magnetic pole could be carried up to 60°.

2567. The crystal of bismuth therefore is able to react upon and affect the magnet at a distance.

2568. But though it thus take up the character of a force acting at a distance, still it is due to that power of the particles which makes them cohere in regular order, and gives the mass its crystalline aggregation; which we call at other times the attraction of aggregation, and so often speak of as acting at *insensible* distances.

2569. For the further explication of the nature of this force, I proceeded to examine the effect of heat on crystals of bismuth when in the magnetic field. The crystals were suspended either by platina or fine copper wire, and heated, sometimes by a small spirit-lamp flame applied directly, sometimes in an oil-bath placed between the magnetic poles; and though the upward currents of air and fluid were strong in these cases, they were far too weak to overcome the set caused by magnecrystallic action, and helped rather to show when that action was weakened or ceased.

2570. When the temperature was gradually raised in the air the bismuth crystal continued to point, until of a sudden it became indifferent in that respect, and turned in any direction under the influence of the rising currents of air. Instantly removing the lamp flame the bismuth revolved slowly and regularly, as if there were no tendency to take up one position more than another, or no remains of magnecrystallic

action; but in a few seconds, as the temperature fell, it resumed its power of pointing; and, apparently, in an instant and with full force, and the pointing was precisely in the same direction as at first. On examining the crystal carefully, its external shape and its cleavage showed that, as a crystal, it was unchanged; but the appearance of a minute globule of bismuth, which had exuded upon the surface in one place, showed that the temperature had been close upon the point of fusion.

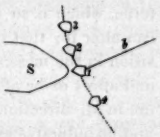
2571. The same result occurred in the oil-bath, except that as removing the lamp from the oil-bath did not immediately stop the addition of heat to the bismuth, so more of the latter was melted; and about one-fourth of the metal appeared as a drop hanging at the lower part. Still the whole mass lost its power at the high temperature, and the power was regained in the same direction, but in a less degree on cooling. The diminished force was accounted for on breaking up the crystal; for the parts which had been liquefied were now crystallized irregularly, and therefore, though active at the beginning of the experiment, were neutral at the end.

2572. As heat has this effect, the expectation entertained (2502.) of crystallizing bismuth regularly *in the magnetic field* is of course unfounded; for the metal must acquire the solid state, and be lowered through several degrees probably, before it can exhibit the magnecrystallic phenomena. If heat has the same effect on all bodies prior to their liquefaction, then, of course, such a process can be applied to none of them.

2573. A crystallized piece of antimony was subjected to the same experiment, and it also lost its magnecrystallic power below a dull red heat, and just as it was softening so as to take the impression of the copper loop in which it was hung. On being cooled it did not resume its former state, but then became ordinarily magnetic and pointed. This I conclude arose from iron affected by the flame and heat of the spirit-lamp; for, as the heat was high enough to burn off part of the antimony and make it rise in fumes of oxide of antimony, so this might set a certain portion of iron free which the carbon and hydrogen of the flame would leave in a very magnetic state (2608.).

2574. In further elucidation of the mutual action of the bismuth and the magnet, the bismuth was suspended, as already described (2551.), on the bifilar balance, but so turned that its magnecrystallic axis, being horizontal, was not parallel or perpendicular to the arm of the lever, but a little inclined, as in the figure (5.), where 1 represents the crystal of bismuth attached to the balance arm *b*, the axis of which is so placed that the crystal can swing through the various positions 1, 2, 3, 4; S is the pole of the magnet separated only by the glass of the shade. It is manifest that in position 1 the magnecrystallic axes and the lines of magnetic force are parallel to each other; whereas in the positions 2, 3, 4, they are oblique. When the apparatus was so arranged that the crystal of bismuth rested at 1, the superinduction of the full magnetic force sent it towards 4; a result of diamagnetic action. When however the

Fig. 5.



bismuth had its place of rest at 2, the development of the magnetic force did not make it pass towards 3, in accordance with the former result, but towards 1, which it usually attained and often passed, going a little towards 4. In this case the magnecrystallic and the diamagnetic forces were opposed to each other, and the former gained the advantage up to position 1.

2575. But though the crystal of bismuth in these cases moves across the lines of force in the magnetic field, it cannot be expected to do so in a field where the lines are parallel and of equal force, as between flat-faced poles; the crystal being restrained so as to move only parallel to itself; for under such circumstances the forces are equal in both directions and on both sides of the mass, and the only tendency the crystal has, in relation to its magnecrystallic condition, is to turn round a vertical axis until it is in its natural position in the magnetic field.

2576. A most important question next arises in relation to the magnecrystallic force, namely, whether it is an original force inherent in the crystal of bismuth, &c., or whether it is induced under the magnetic and electric influences. When a piece of soft iron is held in the vicinity of a magnet it acquires new powers and properties; some persons assume this to depend upon the development by induction of a new force in the iron and its particles, like in nature to that in the inducing magnet: by others it is considered that the force originally existed in the particles of the iron, and that the inductive action consisted only in the arrangement of all the elementary forces in one general direction. Applying this to the crystal of bismuth, we cannot make use of the latter supposition in the same manner; for all the particles are arranged beforehand, and it is that very arrangement of them and their forces which gives the bismuth its power. If the particles of a substance be in the heterogeneous condition possessed by those of the iron in its unmagnetic state, then the magnetic force may develope the magnetic, and also the diamagnetic condition, which probably is a condition of induction; but it does not appear at once, that it can develope a state of the kind now under consideration.

2577. That the particles hold their own to a great extent in all the results is manifest, by the consideration that they have an inherent power or force, the crystalline force, which is so unchangeable that no treatment to which they can be subjected can alter it; that it is this very force which, placing the particles in a regular position in the mass, enables them to act jointly on the magnet or the electric current, and affect or be affected by them; and that if the particles are not so arranged, but are in all directions in the mass, then the sum of their forces externally is nothing, and no inductive exertion of the magnet or current can develope the slightest trace of the phenomena.

2578. And that particles even before crystallization can act in some degree at a distance, by virtue of their crystallizing force, is, I think, shown by the following

fact. A jar containing about a quart of solution of sulphate of soda, of such strength as to crystallize when cold by the touch of a crystal of the salt or an extraneous body, was left, accidentally, for a week or more unattended to and undisturbed. The solution remained fluid; but on the jar being touched, crystallization took place throughout the whole mass at once, producing clear, distinct, transparent plates, which were an inch or more in length, up to half an inch in breadth; and very thin, perhaps about the one-fiftieth or one-sixtieth of an inch. These were all horizontal, and of course parallel to each other; and I think, if I remember rightly, had their length in the same direction; and they were alike in character, and, apparently, in quantity in every part of the jar. They almost held the fluid in its place when the jar was tilted; and when the liquid was poured off presented a beautiful and uniform assemblage of crystals. The result persuaded me, at the time, that, though the influence of a particle in solution and about to crystallize, must be immediately and essentially upon its neighbours, yet that it could exert an influence beyond these, without which influence, the whole mass of solution could hardly have been brought into such a uniform crystallizing state. Whether the horizontality of the plates can have any relation to the almost vertical lines of magnetic force, which from the earth's magnetism was pervading the solution during the whole time of its rest, is more than I will venture to say.

2579. The following are considerations which bear upon this great question (2576.) of an original or an induced state.

2580. In the first place, the bismuth carries off no power or particular state from the magnetic field, able to make it affect a magnet (2504.); so that if the condition acquired by the crystal be an induced condition, it is probably a transient one, and continues only whilst under induction. The fact therefore, though negative in its evidence, agrees, as far as it tells, with that supposition.

2581. In the next place, if the effect were wholly due, as far as the crystal is concerned, to an original power inherent in the mass, we might expect to find the earth's magnetism, or any weak magnet, affecting the crystal. It is true that a weak magnetic force ought to *induce* any given condition in a crystal of bismuth just as well as a stronger, only proportionally. But if the given condition were inherent in the crystal, and did not change in its amount by the degree of magnetic force to which it was subjected, then a weak magnetic force ought to act more decidedly on the bismuth than it would do if the condition were induced in the bismuth, and only in proportion to its own force. Whatever the value of the argument, I was induced to repeat the experiment of the earth's influence (2505.) very carefully, and by sheltering the suspended crystals in small flasks or jar contained within the larger covering jar, and making the experiment in an underground place of uniform and constant temperature, I was able to exclude every effect of currents of air, so that the crystals obeyed the slightest degree of torsion given to the suspending fibre by the index above. Under these circumstances I could obtain no indications of pointing by

the earth's action, either with crystals of bismuth or of sulphate of iron. Perhaps at the equator, where the lines of force are horizontal, they might be rendered sensible.

2582. In the third place, assuming that there is an original force in the crystals and their molecules, it might be expected that they would show some direct influence upon each other, independent of the magnetic force, and if so the best possible argument would be thus obtained that the force which is rendered manifest in the magnetic field was inherent in them. But on placing a large crystal with its magnecrystallic axis horizontal under a smaller and suspended one, or side by side with it, I could procure no signs of mutual action; even when the approximated parts of the crystals were ground or dissolved away, so as to let the two masses come as near as possible to each other, having large surfaces at the smallest possible distance. Extreme care is required in such experiments (2581.), or else many results are produced which seem to show a mutual affection of the bodies.

2583. Neither could I find any trace of mutual action between crystals of bismuth, or of sulphate of iron, when they were both in the *magnetic field*, the one being freely suspended and the other brought in various positions near to it.

2584. From the absence therefore or extreme weakness of any power in the crystals to affect each other, and also from the action of heat which can take away the power of the crystal before it has lost its mere crystalline condition (2570.), I am induced to believe that the force manifested in the crystal when in the magnetic field, which appears by external actions, and causes the motion of the mass, is chiefly and almost entirely *induced*, in a manner, subject indeed to the crystalline force, and finally additive to it; but at the same time exalting the force and the effects to a degree which they could not have approached without the induction.

2585. In that case the word magnetocrystallic ought probably to be applied to this force, as it is generated or developed under the influence of the magnet. The word magnecrystallic I used purposely to indicate that which I believed belonged to the crystal itself, and I shall still speak of the magnecrystallic axis, &c. in that sense.

2586. This force appears to me to be very strange and striking in its character. It is not polar, for there is no attraction or repulsion. Then what is the nature of the mechanical force which turns the crystal round (2460.), or makes it affect a magnet (2564.)? It is not like a turning helix of wire acted on by the lines of magnetic force; for there, there is a current of electricity required, and the ring has polarity all the time and is powerfully attracted or repelled*.

2587. If we suppose for a moment that the axial position is that in which the crystal is unaffected, and that it is in the oblique position that the magnecrystallic axial direction is affected and rendered polar, giving two tensions pulling the crystal round, then there ought to be attractions at these times, and an obliquely

* Perhaps these points may find their explication hereafter in the action of contiguous particles (1663. 1710. 1729. 1735. 2443.).

presented crystal ought to be attracted by a single pole, or the nearest of two poles; but no action of this kind appears.

2588. Or we might suppose that the crystal is a little more apt for magnetic induction, or a little less apt for diamagnetic induction, in the direction of the magnecrystallic axis than in other directions. But, if so, it should surely show polar attractions in the case of the magnetic bodies, as sulphate of iron (2557. 2583.); and in the case of diamagnetic bodies, as bismuth, a difference in the degree of repulsion when presented with the magnecrystallic axis parallel and perpendicular to the lines of magnetic force (2552.); which it does not do.

2589. I do not remember heretofore such a case of force as the present one, where a body is brought into position only, without attraction or repulsion.

2590. If the power be induced, it must be like, generally, to its inducing predominants; and these are, at present, the magnetic and electric forces. If induced, subject to the crystalline force (2577.), it must show an intimate relation between it and them. How hopeful we may be, therefore, that the results will help to throw open the doors which may lead us to a full knowledge of these powers (2146.), and the combined manner in which they dwell in the particles of matter, and exert their influence in producing the wonderful phenomena which they present.

2591. I cannot resist throwing forth another view of these phenomena which may possibly be the true one. The lines of magnetic force may perhaps be assumed as in some degree resembling the rays of light, heat, &c.; and may find difficulty in passing through bodies and so be affected by them, as light is affected. They may, for instance, when a crystalline body is interposed, pass more freely, or with less disturbance, through it in the direction of the magnecrystallic axis than in other directions. In that case, the position which the crystal takes in the magnetic field with its magnecrystallic axis parallel to the lines of magnetic force, may be the position of no, or of least resistance; and therefore the position of rest and stable equilibrium. All the diametral effects would agree with this view. Then, just as the optic axis is to a ray of polarized light, namely, the direction in which it is not affected, so would the magnecrystallic axis be to the lines of magnetic force. If such were the case, then, also, as the phenomena are developed in crystalline bodies, we might hope for the discovery of a series of effects dependent upon retardation and influence in direction, parallel to the beautiful phenomena presented by light with similar bodies. In making this supposition, I do not forget the points of inertia and momentum; but such an idea as I can form of inertia does not exclude the above view as altogether irrational. I remember too, that, when a magnetic pole and a wire carrying an electric current are fastened together, so that one cannot turn without the other, if the one be made axis the other will revolve round and carry the first with it; and also, that if a magnet be floated in mercury and a current sent down it, the magnet will revolve by the powers which are *within* its mass. With my imperfect mathematical knowledge, there seems as much difficulty in these motions as in the

one I am supposing, and therefore I venture to put forth the idea*. The hope of a polarized bundle of magnetic forces is enough of itself to make one work earnestly with such an object, though only in imagination, before us; and I may well say that no man, if he take industry, impartiality and caution with him in his investigations of science, ever works experimentally in vain.

2592. I have already referred, in the former paper (2469.), to PLÜCKER's beautiful discovery and results in reference to the repulsion of the optic axis† of certain crystals by the magnet, and have distinguished them from my own obtained with bismuth, antimony and arsenic, which are not cases of either repulsion or attraction; believing then, with PLÜCKER, that the force there manifested is an optic axis force, exerted in the equatorial direction; and therefore existing in a direction at right angles to that which produces the magnecrystallic phenomena.

2593. But the relations of *both* to crystalline structure, and therefore to the force which confers that condition, are most evident. Other considerations as to position, set, and turning, also show that the two forces, so to say, have a very different relation to each other to that which exists between them and the magnetic or diamagnetic force. As, therefore, this strong likeness on the one hand, and distinct separation on the other is clearly indicated, I will endeavour to compare the two sets of effects, with the view of ascertaining whether the force exerted in producing them is not identical.

2594. I had the advantage of verifying PLÜCKER's results under his own personal tuition in respect of tourmaline, staurolite, red ferro-prussiate of potassa, and Iceland spar. Since then, and in reference to the present inquiry, I have carefully examined calcareous spar, as being that one of the bodies which was at the same time free from magnetic action, and so simple in its crystalline relations as to possess but one optic axis.

2595. When a small rhomboid, about 0·3 of an inch in its greatest dimension, is suspended, with its optic axis horizontal, between the pointed poles (2458.) of the electro-magnet, approximated as closely as they can be, to allow free motion, the rhomboid sets in the equatorial direction, and the optic axis coincides with the magnetic axis; but, if the poles be separated to the distance of half, or three-quarters of an inch, the rhomboid turned through 90°, and set with the optic axis in the equatorial direction, and the greatest length axial. In the first instance the diamagnetic force overcame the optic axis force; in the second the optic axis force was the stronger of the two.

2596. To remove the diamagnetic effect I used flat poles (2463.), and then the little rhomboid always set in, or vibrated about, that position in which its optic axis was equatorial.

* See note (2639.) at the end.

† On the Repulsion of the Optic Axes of Crystals by the Poles of a Magnet, POGGENDORFF's Annalen, vol. lxxii., October 1847, or TAYLOR's Scientific Memoirs, vol. v. p. 353.

2597. I also took three cubes of calcareous spar (1695.), in which the optic axes were perpendicular to two of the faces, of the respective dimensions of 0·3, 0·5, and 0·8 of an inch in the side, and placed these in succession in the magnetic field, between either flat or pointed poles. In all cases, the optic axis, if horizontal, passed into the equatorial position; or, if vertical, left the cubes indifferent as to direction. It was easy by the method of two positions (2470.) to find the line of force, which, being vertical, left the mass unaffected by the magnet; or, being horizontal, went into the equatorial position; and then examining the cube by polarized light, it was found that this line coincided with the optic axis.

2598. Even the horse-shoe magnet (2485.) is sufficiently strong to produce these effects.

2599. I tried two similar cubes of rock-crystal (1692.), but could perceive no traces of any phenomena having either magneoptic, or magnecrystallic, or any other relation to the crystalline structure of the masses.

2600. But though it is thus very certain that there is a line in a crystal of calcareous spar coinciding with the optic axis, which line seems to represent the resultant of the forces which make the crystal take up a given position in the magnetic field; and, though it is equally certain that this line takes up its position in the equatorial direction; yet, considered as a line of force, *i. e.* as representing the direction of the force which places the crystal in that position, it seems to me to have something anomalous in its character. For, that a directing and determining *line* of force should have, as its full effect, the result of going into a *plane* (the equatorial), in which it can take up any one of an infinite number of positions indifferently, leaves an imperfect idea on my mind; and a thought, that there is some other effect or residual phenomena to be recognized and accounted for.

2601. On further consideration, it appears that a simple combination of the magnecrystalline condition, as it exists in bismuth, will supply us with a perfect representation of the state of calcareous spar; for, by placing two equal pieces of bismuth with their magnecrystallic axes perpendicular to each other (2484.), we have a system of forces which seems to possess, as a resultant, a line setting in the equatorial direction. When that line is vertical the system is, as regards position, indifferent; but when horizontal, the system so stands, that the line is in the equatorial plane. Still, the real force is not in the equatorial direction, but axial; and the system is moved by what may be considered a *plane of axial force* (resulting from the union of the two axes at right angles to each other), rather than by a *line of equatorial force*.

2602. Doubtless, the rhomboid or cube (2597.) of calcareous spar is not a compound crystal, like the system of bismuth crystals just referred to (2601.); but its molecules may possess a compound disposition of their forces, and may have two or more axes of power, which at the same time that they cause the crystalline structure, may exert such force in relation to the magnet, as to give results in the same manner, and of the same kind, as those of the double crystal of bismuth (2601.). Indeed,

that there should be but one axis of crystalline force, either in the particle of Iceland spar, or in those of bismuth, does not seem to me to be any way consistent with the cleavage of the substances in three or more directions.

2603. The optic axis in a piece of calcareous spar, is simply the line in which, if a polarized, or ordinary ray of light moves, it is the least affected. It may be a line which, as a resultant of the molecular forces, is that of the least intensity; and, certainly, as regards ordinary and mechanical means of observing cohesion, a piece of calcareous spar is sensibly, and much harder on the faces and parts which are parallel to the optic axis, than on those perpendicular to it. An ordinary file or a piece of sandstone shows this. So that the plane equatorial to the optic axis, as it represents directions in which the force causing crystallization is greater in degree than in the direction of the optic axis, may also be that in which the resultant of its magnecrystallic force is exerted.

2604. I am bound to state, as in some degree in contrast with such considerations, that, with bismuth, antimony and arsenic, the cleavage is very facile perpendicular to the magnecrystallic axis (2475. 2510. 2532.). But we must remember that the cleavage (and therefore the cohesive) force is not the only thing to be considered, for in calcareous spar it does not coincide with either the axial or the equatorial direction of the substance in the magnetic field: we must endeavour to look beyond this to the polar (or axial) condition of the particles of the masses, for the full understanding and true relation of all these points.

2605. I am bound, also, to admit that, if we consider calcareous spar as giving the simple system of force, we may, by the jaxtaposition of two crystals with their optic axes at right angles to each other, produce a compound mass, which will truly represent the bismuth in the direction of the force; *i. e.* it will, in the magnetic field, point with apparently one line of force only, and that in the axial direction, whilst it may be really moved by a system of forces lying in the equatorial plane. I will not at present pretend to say that this is not the state of things; but I think, however, that the metals, bismuth, antimony and arsenic, present us with the simplest as they do the strongest cases of magnecrystallic force; and whether that be so or not I am still of opinion that the phenomena discovered by PLÜCKER and those of which I have given an account in these two papers, have one common origin and cause.

2606. I went through all the experiments and reasonings with PLÜCKER's crystals (as the carbonate of lime, tourmaline and red ferro-prussiate of potassa), in reference to the question of original or induced power (2576.), as before, and came to the same conclusion as in the former case (2584.).

2607. I could not find that crystals of red ferro-prussiate of potassa or tourmaline were affected by the earth's magnetism (2581.), or that they had the power of affecting each other (2582.). Neither could I find that PLÜCKER's effect with calcareous spar, or red ferro-prussiate of potassa, was either an attractive or repulsive effect, but one connected with position only (2550. 2560.). All which circumstances tend to

convince me that the force active in his experiments, and that in my results with bismuth, &c., is the same*.

2608. A small rhomboid of Iceland spar was raised to the highest temperature in the magnetic field which a spirit-lamp could give (2570.); it was at least equal to the full red heat of copper, but it pointed as well then as before. A short thick tourmaline was heated to the same degree, and it also pointed equally well. As it cooled, however, it became highly magnetic, and seemed to be entirely useless for experiments at low temperatures; but on digesting it for a few seconds in nitromuriatic acid, a little iron was dissolved from the surface, after which it pointed as well, and in accordance with PLÜCKER's law, as before. A little peroxide upon the surface had been reduced by the flame and heat to protoxide, and caused the magnetic appearances.

2609. There is a general and, as it appears to me, important relation between PLÜCKER's magneto-optical results and those I formerly obtained with heavy glass and other bodies (2152, &c.). When any of these bodies are subject to strong induction under the influence of the magnetic or electric forces, they acquire a peculiar state, in which they can influence a polarized ray of light. The effect is a rotation of the ray, if it be passed through the substance parallel to the lines of magnetic force, or in other words, in the axial direction; but if it be passed in the equatorial direction, no effect is produced. The equatorial plane, therefore, is that plane in which the condition of the molecular forces is the least disturbed as respects their influence on light. So also in PLÜCKER's results, the optic axis, or the optic axes, if there be two, go into that plane under the same magnetic influence, they also being the lines in which there is the least, or no action on polarized light.

2610. If a piece of heavy glass, or a portion of water, could be brought beforehand into this constrained condition, and then placed in the magnetic field, I think there can be no doubt that it would move, if allowed to do so, and place itself naturally, so that the plane of no action on light should be equatorial, just as PLÜCKER shows that a crystal of calcareous spar or tourmaline does in his experiments. And, as in his case, the magnetic or diamagnetic character of the bodies, makes no difference in the general result; so in my experiments, the optical effect is produced in the same direction, and subject to the same laws, with both classes of substances (2185. 2187.).

2611. But though thus generally alike in this great and leading point, there is still a vast difference in the disposition of the forces in the heavy glass and the crystal;

* The optic axis is the direction of least optic force; and by PLÜCKER's experiments, coincides with what I consider in my results as the direction of minimum magnecrystallic force. It is more than probable that, wherever the two sets of effects (whether really or only nominally different) can be recognized in the same body, the directions of maximum effect, and also those of minimum effect, will be found to coincide.—November 23, 1848.

and there is a still greater difference in this, that the heavy glass takes up its state only for a time by constraint and under induction, whilst the crystal possesses it freely, naturally and permanently. In both cases, however, whether natural or induced, it is a state of the particles; and comparing the effect on light of the glass under constraint with that of the crystal at liberty, it indicates a power in the magnet of inducing something like that condition in the particles of matter which is necessary for crystallization; and that even in the particles of fluids (2184.).

2612. If there be any weight in these considerations, and if the forces manifested in the crystals of bismuth and Iceland spar be the same (2607.), then there is further reason for believing that, in the case of bismuth and the other metals named, there is, when they are subjected to the power of the magnet, both an induced condition of force (2584.), and also a pre-existing force (2577.). The latter may be distinguished as the crystalline force, and is shown, first, by such bodies exhibiting optic axes and lines of force when not under induction; by the symmetric condition of the whole mass, produced under circumstances of ordinary occurrence; and by the fixity of the line of magnecrystallic force in the bodies shown experimentally to possess it.

2613. Though I have spoken of the magnecrystallic axis as a given line or direction, yet I would not wish to be understood as supposing that the force decreases, or state changes, in an equal ratio all round from it. It is more probable that the variation is different in degree in different directions, dependent on the powers which give difference of form to the crystals. The knowledge of the disposition of the force can be ascertained minutely hereafter, by the use of good crystals, an unchangeable ordinary magnet (2485. 2528.), or a regulated electro-magnet, flat-faced poles (2463.), and torsion (2500. 2530.).

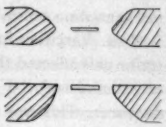
2614. I cannot conclude this series of researches without remarking how rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develope more and more their importance, and their extreme attraction as an object of study. A few years ago magnetism was to us an occult power, affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relations with electricity, heat, chemical action, light, crystallization, and, through it, with the forces concerned in cohesion; and we may, in the present state of things, well feel urged to continue in our labours, encouraged by the hope of bringing it into a bond of union with gravity itself.

Royal Institution,
October 20, 1848.

¶ vi. NOTE.—*On the position of a crystal of sulphate of iron in the magnetic field.*

Received December 7, 1848.—Read December 7, 1848.

Fig. 6.



2615. Though effects of the following nature are general, yet I think it convenient to state that I obtained them chiefly by the use of magnetic poles (2247.), the form of which is given in the plan and side view annexed (fig. 6.). The crystals submitted to their action were suspended by cocoon silk, so as to be level with the upper surface of the poles.

2616. A prismatic crystal of proto-sulphate of iron was selected, which was nearly 0·9 of an inch in length, 0·1 in breadth, and 0·05 in thickness; by examination the magnecrystallic axis was found to coincide with the thickness, and therefore to be perpendicular, or nearly so (2546.), to the plate. Being suspended as above described, and the magnet (2247.) excited by ten pair of GROVE's plates, the crystal stood transverse, or with its *magnecrystallic axis* parallel to the axis of magnetic force, when the distance between the poles was 2·25 inches or more; but when the distance was about two inches or less, then it stood with its length axial, or nearly so, and its magnecrystallic axis therefore transverse to the lines of magnetic force. In the intermediate distances between 2 and 2·25 inches, the prism assumed an oblique position (2634.), more or less inclined to the axial line, and so passing gradually from the one position to the other. This intermediate distance I will for the present call *n* (neutral) distance.

2617. If the poles be two inches apart and the crystal be gradually lowered, it passes through the same intermediate oblique positions into the transverse position; or if the crystal be raised, the same transitions occur; at any less distance the changes are the same, but later. They occur more rapidly when the crystal is raised than when it is lowered; but this is only because of the unsymmetric disposition and intensity of the lines of magnetic force around the magnetic axis, due to the horse-shoe form of the magnet and shape of the poles. If two cylinder magnets with equal conical terminations were employed, there is no doubt that for equal amounts of elevation or depression, corresponding changes would take place in the position of the crystal.

2618. These changes however are not due to mere diminution of the magnetic force by distance, but to differences in the *forms* or *direction* of the resultants of force. This is shown by the fact that, if the crystal be left in its first position, and so pointing with the length axially, no diminution of the force of the magnet alters the position; thus, whether one or ten pair of plates be used to excite the magnet, the *n* distance (2616.) remains unchanged; and even descending to the use of an ordinary horse-shoe magnet, I have found the same result.

2619. Variation in the length of the prismatic crystal has an important influence

over the result. As the crystal is shorter the distance *n* diminishes, all the other phenomena remaining the same. A crystal 0·7 of an inch long, but thicker than the last, had for its maximum *n* distance 1·7 inch. A still shorter crystal had for its maximum *n* distance 1·1 inch. In all these cases variation of the force of the magnet caused no sensible change.

2620. Variation in that dimension of the crystal coincident with the magnecrystallic axis affected the *n* distance: thus, increase in the length of the magnecrystallic axis diminished the distance, and diminution of it in that direction increased the distance. This was shown in two ways; first, by placing a second prismatic crystal by the side of the former in a symmetric position (2636.), which reduced the *n* distance to between 1·75 and 2 inches; and next, by employing two crystals in succession of the same length but different thicknesses. The thicker one had the smaller *n* distance.

2621. Variation in the depth of the crystal, *i. e.* its vertical dimension, did not produce any sensible effect on the *n* distance: nor by theory should it do so, until the extension upwards or downwards brings the upper or lower parts into the condition of raised or depressed portions (2617.).

2622. Variation in the form of the poles affects the *n* distance. As they are more acute, the distance increases; and as they are more obtuse up to flat-faced poles (2463.), the distance diminishes.

2623. With the shorter crystals, or with obtuse poles, it is often necessary to diminish the power of the magnet, or else the crystal is liable to be drawn to the one or other pole. This, however, may be avoided by employing a vertical axis which is confined below as well as above (2554.); and then the difference in *strength* of the magnet is shown to be indifferent to the results, or very nearly so.

2624. These effects may probably be due to the essential difference which exists between the ordinary magnetic and the magnecrystallic action, in that the first is *polar*, and the second only *axial* (2472.) in character. If a piece of magnetic matter, iron for instance, be in the magnetic field, it immediately becomes polar (*i. e.* has terminations of different qualities). If many iron particles be there, they all become polar; and if they be free to move, arrange themselves in the direction of the axial line, being joined to each other by contrary poles; and by that the polarity of the extreme particles is increased. Now this does not appear to be at all the case with particles under the influence of the magnecrystallic force; the force seems to be altogether *axial*, and hence probably the difference above, and in many other results.

2625. Thus, if four or more little cubes of iron be suspended in a magnetic field of equal force (2465.), they will become polar; if also four similar cubes of crystallized bismuth be similarly circumstanced, they will be affected and point. If the iron cubes be arranged together in the direction of the equatorial line, they will form

an aggregate in a position of unstable equilibrium, and will immediately, as a whole, turn and point with the length axially; whereas the bismuth cubes by such approximation will suffer no sensible change.

2626. The *extreme* (and the other) associated cubes of the elongated iron arrangement now have a polar force above that which they had before; and the whole group serves, as it were, as a conductor for the lines of magnetic power; for many of them concentrate upon the iron, and the intensity of power is much stronger between the ends of the iron arrangement and the magnetic poles, than it is in other parts of the magnetic field. Such is not the case with the bismuth cubes; for however they be arranged, the intensity of force in the magnetic field is, as far as experiments have yet gone, unaffected by them; and the intensity of the molecules of the crystals appears to remain the same. Hence the iron stands lengthways between the poles; the bismuth crystals, on the contrary, whether arranged side by side, as respects the magnecrystallic axis, so as to stand as to length equatorially; or end to end, so as to stand axially, are perfectly indifferent in that respect, vibrating and setting equally both ways.

2627. A given piece of iron when introduced into a field of equal magnetic force, and brought towards the pole, adheres to it and disturbs the intensity of the field, producing a pointed form of pole in one part with diverging lines of force: a crystal of bismuth vibrates with sensibly equal force in every part of the field (2467.), and does not disturb the distribution of the power.

2628. Considering all these actions and conditions, it appears to me that the occurrence of the *n* distance with a body which is at the same time magnetic and magnecrystallic, may be traced to that which causes them and their differences, namely, the *polarity* belonging to the magnetic condition, and the *axiality* belonging to the magnecrystallic condition. Thus, suppose an uniform magnetic field three inches from pole to pole, and a bar of magnetic matter an inch long, suspended in the middle of it; by virtue of the polarity it acquires, it will point axially, and carry on, or conduct, with its mass, the magnetic force, so much better than it was conducted in the same space before, that the lines of force between the ends of this bar and the magnetic poles, will be concentrated and made more intense than anywhere else in the magnetic field. If the poles be made to approach towards the bar, this effect will increase, and the bar will conduct more and more of the magnetic force, and point with proportionate intensity. It is not merely that the magnetic field becomes more intense by the approximation of the poles, but the proportion of force carried on by the bar becomes greater as compared to that conveyed onwards by an equal space in the magnetic field at its side.

2629. But if a similar bar of magnecrystallic substance be placed in the magnetic field, its power does not rise in the same manner, or in the same great proportion, by approximation of the poles. There can be no doubt that such approximation increases the intensity of the lines of force, and therefore increases the intensity of

the magneto-crystallic state; but this state does not appear to be due to polarity, and the bar does not convey more power through it than is conveyed onwards elsewhere through an equal space in the magnetic field. Hence its directive force does not increase in the same rapid degree as the directive force of the magnetic bar just referred to.

2630. If then we take a bar which, like a prism of sulphate of iron, is magnetic, and also magnecrystallic, having the magnecrystallic axis perpendicular to its length, such a bar, properly suspended, ought to have an *n* distance of the poles, within which the forces ought to be nearly in equilibrium; whilst at a greater distance of the poles, the magnecrystallic force ought to predominate; and at a lesser distance, the magnetic force ought to have the advantage; simply, because the magnetic force, in consequence of the true polarity of the molecules, grows up more rapidly and diminishes more rapidly than the magneto-crystallic force.

2631. This view, also, is consistent with the fact that variation of the force of the magnet does not affect the *n* distance (2618. 2619.); for, whether the force be doubled or quadrupled, both the magnetic and magneto-crystallic forces are at the same time doubled or quadrupled; and their proportion therefore remains the same.

2632. The raising or lowering of the crystal above or below the line of maximum magnetic force is manifestly equivalent in principle to the separation of the magnetic poles; and therefore should produce corresponding effects: and that is the case (2617.). Besides that, when the crystal is raised above the level of the poles, such resultants of magnetic force as pass through it, are no longer parallel to its length, but more or less curved, so that they probably cannot act with the same amount of power in throwing the whole crystal into a consistent polarized magnetic condition, as if they were parallel to it: whereas, as respects the induction of the magneto-crystallic condition, each of the particles appears to be affected independently of the others; and, therefore, any loss of an effect dependent upon joint action would not be felt here.

2633. M. PLÜCKER told me, when in England in August last, that the repulsive force on the optic axis diminishes and increases less rapidly than the magnetic force, by change of distance; but is not altered in its proportion to the magnetic force by employing a stronger or weaker magnet. This is manifestly the same effect as that I have been describing; and makes me still more thoroughly persuaded that his results and mine are due to one and the same cause (2605. 2607.).

2634. I have said that, within the *n* distance, the crystal of sulphate of iron pointed more or less obliquely (2616.); I will now state more particularly what the circumstances are. If the distance *n* be so adjusted, that the prismatic crystal, which is at the time between the magnetic poles, shall make an angle of 30° (or any quantity) with the axial line; then it will be found that there is another stable position, namely, the diametral position (2461.), in which it can stand; but that the obliquity

is always on the same side of the axial line; and that the crystal will not stand with the like obliquity of 30° on the opposite side of the magnetic axis.

2635. If the crystal be turned 180° round a vertical axis, or end for end, then the inclination, and the direction in which it occurs, remain unchanged; in fact, it is simply giving the crystal the diametral position. But if the crystal be revolved 180° round a horizontal axis; either that coinciding with its length, which represents its maximum magnetic direction; or that corresponding with its breadth, and therefore with the magnecrystallic axis; then the inclination is the same in amount as before, but it is on the *other* side of the axial line.

2636. This is the case with all the prismatic crystals of sulphate of iron which I have tried. The effect is very determinate; and, as would be expected, when two crystals correspond in the direction of the inclination, they also correspond in the position of their form and direction of the various planes.

2637. All these variations of position indicate an oblique resultant of setting force, derived from the joint action of the magnetic and magnecrystallic forces; and would be explained by the supposition, that the magnecrystallic axis or line of maximum magnecrystallic force, was not perpendicular to the chief planes of the crystal (or those terminating it), but a little inclined in the direction of the length.

2638. Whether this be the case, or whether the maximum line of magnetic force may not, even, be a little inclined to the length of the prism; still, the n distance supplies an excellent experimental opportunity of examining this inclination, however small its quantity may be; because of the facility with which the influence of either the one or the other may be made predominant in any required degree.

Royal Institution,
December 5, 1848.

2639. *Note.* (2591.) Another supposition may be thrown out for consideration. I have already said that the assumption of a mere axial condition (2587. 2591.) would account for the set without attraction or repulsion. Now if we suppose it possible that the molecules should become polar in relation to the north and south poles of the magnet, but with *no mutual relation amongst themselves*, then the bismuth or other crystal might set as if induced with mere axial power: but it seems to me very improbable that polarities of a given particle in a crystal should be subject to the influence of the polarities of the distant magnet poles, and not also to the *like polarities* of the contiguous particles.—January 24, 1849.

III. *On the Ganglia and Nerves of the Heart.* By Robert Lee, *M.D., F.R.S., Fellow of the Royal College of Physicians, London.*

Received May 7,—Read June 7, 1848.

Haller, Wrisberg, Soemmering, and other eminent anatomists prior to Scarpa, have affirmed that no nerves are distributed to the muscular substance of the heart, and that its contractions do not depend upon nervous influence.

B. J. Behrends, a pupil of Soemmering, in 1792 published a memoir, entitled "Dissertatio qua demonstratur Cor Nervis carere," in which it is admitted that nerves accompany the coronary arteries, and it is distinctly asserted that the muscular structure is entirely destitute of nerves*.

The elaborate and splendid work of Scarpa, "Tabulæ Neurologicæ," fol. 1794, has for its chief object the refutation of these erroneous views; but before referring to the discoveries of that great authority, I may proceed to state that in the magnificent Plates of Mr. Swan only a few small branches of nerves have been figured, which accompany the trunks of the coronary arteries, and the muscular substance of the heart is represented as almost completely destitute of nerves.

M. Chassaignac, who translated in 1838 Mr. Swan's "Demonstration of the Nerves of the Human Body," has repeatedly denied, in the most positive manner, that any nerves except those which accompany the coronary arteries have yet been demonstrated in the heart. "Anatomie n'a constaté jusqu'à présent, dans le cœur que des nerfs artériels,"—"l'existence de filets nerveux indépendantes des vaisseaux propres au tissu charnu est encore à démontrer." p. 23.

Scarpa, however, had clearly delineated and described such nerves, viz. running on the heart independently of, and distinct from the coronary arteries. In the work above cited, he has given five views of the nerves of the human heart, in some of which, *e. g.* Tab. IV., upwards of twenty filaments may be counted on the same transverse line near the base of the heart, together with numerous anastomotic angular enlargements, which Scarpa does not specify as ganglions in his text. In the hearts of the larger herbivorous Mammals, however, Scarpa describes and delineates both ganglia and fusiform enlargements of the nerves, which he calls corpora olivaria, and these not only upon the nerves at the base of the heart, but upon those that are spread over the superficies of the ventricle: his words are, "Præcipue autem nervorum cardiacorum trunci ad basim cordis et inter majora vasa arteriosa intumescant

* Ac primo quidem nervorum cordis examini scrupulosius intendens, tum observando, tum analogice concludendo didici *nullos* omnino nervos ne surculum quidem in ipsam cordis carnem dispergi.

in vera et genuina ganglia; in Equo autem et Bove etiam in iis ramis cardiacorum qui per cordis superficiem reptant nonnulla corpora olivaria gignunt*." In Tab. VII. fig. 1, he represents, and at p. 42 specifies some of these enlargements; one, *e. g.* marked 7, as a "gangliformis intumescentia;" a second, marked 30, as "cardiaci sinistri ganglion insigne." Scarpa also describes and figures several nerves independent of, and not accompanying the blood-vessels of the heart, and avails himself of the fact to refute the conclusions to which Behrends had arrived in the Treatise above quoted.

The following are the facts relative to the nervous supply of the heart which I believe myself to have established by examination of the fœtal heart, of the heart of a child at the age of six years, of the heart of an adult in a sound state, of the human heart hypertrophied, and of the heart of the Ox, and which the preparations are preserved to demonstrate.

The drawing No. 1, entitled "The nerves of the heart of a child nine years of age," nat. size, represents the preparation displaying the nerves distributed over the exterior of the left ventricle which come off from the "plexus coronarius posticus" of Scarpa†, together with a few filaments from the "plexus coronarius anterior," Scarpa. It shows the ganglions which Scarpa has delineated below the letters *a* and *b* in his Tab. IV., and also the slight enlargement at point of confluence of three or more nerves which Scarpa has likewise figured, as *e. g.* between the nerves numbered 58 and 59, and in several other parts of the cardiac nerves displayed in the Tab. IV. above cited. In the place of the long and narrow loop on the nerve which Scarpa figures between the two chief branches of the posterior coronary artery, my preparation shows, as in the drawing herewith sent, a slender fusiform enlargement. The preparation also demonstrates nerves extending beyond the points where they end in Scarpa's figure, as far as the apex of the heart; and a slight expansion and flattening is presented by some of these apicial filaments of nerves, and nerves not coincident in their course with the arterial branches are also shown in the preparation which have neither been described nor delineated by previous anatomists.

In the dissection of the sound heart of the adult, depicted in the drawing No. 2, entitled "The ganglia and nerves at the apex of the left ventricle of the sound human heart," the additional nerves at the apex of the left ventricle are more clearly shown, in which three slender fusiform enlargements are shown on nerves accompanying the apicial branch of the posterior coronary artery: there is also a well-marked angular enlargement at the point of junction of four nerves near a neighbouring branch of the artery.

The preparation which most distinctly establishes the fact of fusiform enlargements of the cardiac nerves, is that represented in the drawing No. 3, entitled "The ganglia and nerves of the left ventricle of a Heifer's heart and cardiac fascia:" in

* *Op. cit.* p. 2.

† Tabulæ Neurologicæ, fol. 1794, Tab. IV. Nos. 45, 46, 47, 48, 60 and 61.

which it will be seen that some of these fusiform ganglionic enlargements of the cardiac nerves are nearly in the same position as that of the "ganglion insigne," described and figured by Scarpa in the heart of the Horse, Tab. VII.

The ventricles and auricles of the human heart and those of the larger quadrupeds are covered with two distinct membranes. The first or exterior of these is the serous membrane which lines the pericardium and is reflected over the whole surface of the heart; this membrane is connected rather firmly by cellular tissue with another tunic, which has scarcely if at all been noticed by anatomists. This second membrane has a dense fibrous structure, is semitransparent, and resembles in a striking manner the aponeurotic expansions or fasciæ covering muscles in other parts of the body, and, like them, sends numerous fibres or processes between the muscular fasciculi, blood-vessels, nerves and adipose substance of the heart, which it binds closely together. This aponeurotic expansion investing both ventricles and auricles may be appropriately termed, from its structure and function, the fibrous membrane, or *Cardiac Fascia*.

The drawings, which have been executed by Mr. West with the greatest pains and attention to accuracy, will supply the need of special verbal description of the nervous filaments, their anastomotic enlargements and fusiform swellings; and the series of my dissections shows that the nerves of the heart which are distributed over its surface, and throughout its walls to the lining membrane and columnæ carneæ, enlarge with the natural growth of the heart, before birth and during childhood and youth, until the heart has attained its full size in the adult; that the nervous supply of the left ventricle is greater than that of the right; and that when the walls of the auricles and ventricles are affected with hypertrophy, the ganglia and nerves of the heart are enlarged like those of the gravid uterus.

Explanation of the Plates.

PLATE I.

Fig. 1 represents the great cardiac ganglionic plexus of nerves, situated between the aorta and pulmonary artery, which receives branches of nerves from the sympathetic, par vagum, and recurrent nerves of both sides: and likewise the ganglia and nerves distributed over the surface of the left ventrical of the heart of a child nine years of age. Natural size.

a. The arch of the aorta.

b. The pulmonary artery truncated at its origin.

c. The anterior surface of the left ventricle of the heart.

d. The anterior surface of the right ventricle.

e. The left par vagum and recurrent nerve.

f. The great cardiac ganglionic plexus of nerves situated between the aorta and pulmonary artery, from which all the principal cardiac nerves are derived.

g. The ganglionic plexus of nerves accompanying and surrounding the trunk and branches of the left coronary artery, and the ganglia and nerves distributed over the muscular substance of the left ventricle to the apex; the serous membrane and cardiac fascia having been removed.

Fig. 2 represents the ganglia and nerves at the apex of the anterior surface of the adult human heart in the natural state, with a portion of the cardiac fascia dissected off from the blood-vessels, nerves and muscular substance to which it firmly adhered.

a. The branches of the coronary artery at the apex of the heart surrounded by ganglia and nerves.

b. Ganglia and nerves on the muscular substance af the heart ot the apex not accompanying blood-vessels.

c. The cardiac fascia.

PLATE II.

Represents a portion of the cardiac fascia, and the ganglia and nerves on the surface of the left ventricle of the Heifer's heart.

a. A portion of the serous membrane dissected off from the cardiac fascia.

b. The cardiac fascia with the numerous ganglia and nerves seen through it, undisturbed by dissection.

c. Branches of the left coronary artery, with ganglia on the nerves where they cross the blood-vessels.

IV. *Postscript to a Paper "On the Ganglia and Nerves of the Heart."*

By Robert Lee, *M.D., F.R.S.*

Received December 21, 1848,—Read January 11, 1849.

SINCE the communication above referred to was presented to the Royal Society, I have made a very minute dissection in alcohol of the whole nervous system of the young heifer's heart. The distribution of the ganglia and nerves over the entire surface of the heart, and the relations of these structures to the blood-vessels and muscular substance, are far more fully displayed in these preparations than in any of my former dissections. On the anterior surface, there are distinctly visible to the naked eye ninety ganglia or ganglionic enlargements on the nerves, which pass obliquely across the arteries and the muscular fibres of the ventricles from their base to the apex. These ganglionic enlargements are observed on the nerves, not only where they are crossing the arteries, but where they are ramifying on the muscular substance without the blood-vessels.

On the posterior surface, the principal branches of the coronary arteries plunge into the muscular substance of the heart near the base, and many nerves with ganglia accompany them throughout the walls to the lining membrane and columnæ carneæ. From the sudden disappearance of the chief branches of the coronary arteries on the posterior surface, the nervous structure distributed over a considerable portion of the left ventricle is completely isolated from the blood-vessels, and on these, numerous ganglionic enlargements are likewise observed, but smaller in size than the chains of ganglia formed over the blood-vessels on the anterior surface of the heart. In the accompanying beautiful drawings, Mr. West has depicted with the greatest accuracy and minuteness the whole nervous structures demonstrable in these preparations on the surface of the heart. But the ganglia and nerves represented in these drawings constitute only a small portion of the nervous system of the heart, numerous ganglia being formed in the walls of the heart which no artist can represent. It can be clearly demonstrated that every artery distributed throughout the walls of the Uterus and Heart, and every muscular fasciculus of these organs, is supplied with nerves upon which ganglia are formed.

Explanation of the Plates.

PLATE III.

Exhibits the trunk and branches of the coronary arteries, and the ganglia and nerves distributed over the anterior surface of the ventricles of the young Heifer's heart; the serous membrane and cardiac fascia having been wholly removed.

PLATE IV.

Represents the posterior surface of the same heart covered with ganglia and nerves, from the base to the apex.

PLATE V.

Represents the aorta and the anterior surface of a human heart which was hypertrophied, and weighed four pounds. The trunk and some of the branches of the left coronary artery were ossified. The pulmonary artery has been cut away close to the right ventricle. A portion of the wall of the right ventricle has been removed to expose the cavity and the septum between the ventricles. The serous membrane has been reflected off from the cardiac fascia, a small portion only of which has been left covering the ventricle.

a. The arch of the aorta.
b. The origin of the pulmonary artery, which has been completely removed.
c. The anterior surface of the left ventricle.
d. The anterior surface of the right ventricle.
e. The great ganglionic plexus of nerves into which branches from the par vagum, recurrent and sympathetic nerves of both sides enter, and from which the principal cardiac nerves take their origin.
f. The par vagum of the left side.
g. The trunk of the left coronary artery ossified and completely surrounded with ganglia and nerves, which are distributed over the whole surface of the ventricle to the apex.
h. The serous membrane reflected off from the cardiac fascia, a small portion only of which is left covering the ganglia and nerves near the apex.
i. The cardiac fascia.

V. *On the Effect of surrounding Media on Voltaic Ignition.*

By W. R. Grove, *Esq.*, *M.A.*, *V.P.R.S.*

Received August 10,—Read December 14, 1848.

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In the Philosophical Magazine for December 1845, I pointed out a striking difference between the heat generated in a platinum wire by a voltaic current, according as the wire is immersed in atmospheric air or in hydrogen gas, and in the Bakerian Lecture for 1847 I have given some further experiments on this subject, in which the wire was ignited in atmospheres of various gases, while a voltameter enclosed in the circuit yielded an amount of gas in some inverse ratio to the heat developed in the wire. It was also shown, by a thermometer placed at a given distance, that the radiated heat was in a direct ratio with the visible heat.

Although the phenomenon was apparently abnormal, there were many known physical agencies by which it might possibly be explained, such as the different specific heats of the surrounding media, their different conducting powers for electricity, or the varying fluency or mobility of their particles which would carry off the heat by molecular currents with different degrees of rapidity.

The investigation of these questions will form the subject of this paper.

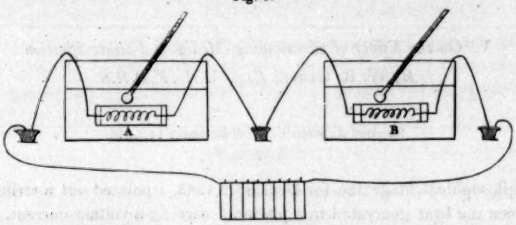
An apparatus was arranged, see fig. 1. Two glass tubes A and B, of 0·3 inch internal diameter and 1·5 inch length, were closed with corks at each extremity; through the corks the ends of copper wires penetrated, and joining these were coils of fine platinum wire, one-eightieth of an inch diameter and 3·7 inches long when uncoiled. Tube A was filled with oxygen, tube B with hydrogen, and the tubes thus prepared were immersed in two separate vessels, in all respects similar to each other, and containing each three ounces of water. A thermometer was placed in the water in each vessel; the copper wires were connected, so as to form a continued circuit, with a nitric acid battery of eight cells, each plate exposing eight square inches of surface. Upon the circuit being completed the wire in the tube containing oxygen rose to a white heat, while that in the hydrogen was not visibly ignited; the temperature of the water, which at the commencement of the experiment was 60° Fahr. in each vessel, rose in five minutes in the water surrounding the tube of hydrogen from 60° to 70°, and in that containing oxygen from 60° to 81°*.

Before I enter into a further detail of experiments, I would remark upon the extraordinary character of this result. The same current or quantity of electricity

* After the publication of the Bakerian Lecture, my experiment on the peculiar effect of hydrogen on the ignited wire was noticed in a paper by M. Matteucci, which though I had it in my hand shortly after its

passes through two similar portions of wire immersed in the same quantity of liquid,

Fig. 1.



and yet, in consequence of their being surrounded by a thin envelope of different gases, a large portion of the heat which is developed in the one portion appears to have been annihilated in the other. Similar experiments, varying the gas in one tube while hydrogen was retained in the other, gave the following results. In five minutes the thermometer rose—

	In the hydrogen.	In the associated nitrogen.
1st.	From 60° to 69°·5.	From 60° to 81°·5.
	In hydrogen.	**In carbonic acid.**
2nd.	From 60° to 70°·5.	From 60° to 80°.
	In hydrogen.	**In carbonic oxide.**
3rd.	From 60° to 70°.	From 60° to 79°·5.
	In hydrogen.	**In olefiant gas.**
4th.	From 60° to 70°·5.	From 60° to 76°·5 *.

On a different day I tried the following experiments; all the circumstances were the same, excepting that the battery was in more energetic action, for which reason I have not tabulated them with the others.

publication, I regret to say I did not read with the attention it deserved. I have read it since the experiments in this paper were commenced, and I see that I am now executing a task assigned to me by my friend. M. Matteucci, for a different object, makes a somewhat similar experiment to the one given above, which however differs from mine in the material point, that he operated first on one gas and then on the other, and thus did not compare the effects produced by the same quantity of electricity. I cannot quite agree in the conclusions deduced by him from this and the other experiments he cites, but I will not here contest them, as it would lead me away from the main point of this paper.

* I should perhaps remark, that several test experiments were tried to ascertain the working of the apparatus; thus, the same gas was placed in both tubes, and the results given by the thermometer were found to be accurately the same in both vessels. The tubes were also changed with reference to the containing vessels and to the contained gases. The water was always agitated to render its temperature uniform previously to reading off, &c. &c.

In oxygen associated with coal gas the thermometer rose in five minutes—

In oxygen.	In coal gas.
From 60° to 82°.	From 60° to 76°.

In hydrogen associated with coal gas the thermometer rose in five minutes—

In hydrogen.	In coal gas.
From 60° to 77°.	From 60° to 82·5°.

From this it would appear that coal gas should be placed, as to its cooling effect on the ignited wire, between hydrogen and olefiant gas.

On another day sulphuretted hydrogen associated respectively with oxygen and hydrogen was tried; the wire in the sulphuretted hydrogen was at first ignited to a degree somewhat inferior to that in oxygen, but the gas was rapidly decomposed; sulphur being deposited on the interior of the vessel and the intensity of ignition gradually decreased, so as ultimately to be scarcely superior to the ignition in hydrogen: indeed the gas by this time had become nearly pure hydrogen. The following were the effects on the thermometer in five minutes, all being arranged as before.

In oxygen.	In sulphuretted hydrogen.
From 60° to 86°.	From 60° to 76°.

In hydrogen.	In sulphuretted hydrogen.
From 60° to 79°.	From 60° to 81°·5.

This result would place sulphuretted hydrogen between hydrogen and coal gas; but as the gas was rapidly decomposed, the greater part of the experiment was made with hydrogen containing small quantities of sulphur combined, and not with sulphuretted hydrogen. I therefore think that proto-sulphuret of hydrogen, or the gas which consists of equivalent ratios of the two elements, would be much further removed from pure hydrogen; probably it would be about equal in its cooling effect to carbonic acid or carbonic oxide.

In phosphuretted hydrogen the platinum wire is destroyed by combining with the phosphorus the instant it reaches ignition, so that its relation to the other gases could not be ascertained.

Protoxide and deutoxide of nitrogen are, as I have observed in the Bakerian Lecture, decomposed by the ignited wire; they, as well as atmospheric air, are, as nearly as may be, equal in their effect to their elements separately.

In the vapour of ether the ignited wire is extinguished nearly as completely as in hydrogen; I have not yet tried its comparative effect, but should judge it to be nearly the same as coal gas or olefiant gas.

In my former experiments* the following was the order of the gases, testing the intensity of ignition by the inverse conducting power of the wire, as measured by the amount of gas in a voltameter included in the circuit.

* Philosophical Transactions, 1847, p. 2.

Gases surrounding the wire.	Cubic inches of gas evolved in the voltameter per minute.
Hydrogen	7·7
Olefiant gas	7·0
Carbonic oxide	6·6
Carbonic acid	6·6
Oxygen	6·5
Nitrogen	6·4

Assuming that in the present experiments the heat in the water is a correct indication of the intensity of ignition in the wire, the order is the same in both series of experiments. Hydrogen is however so far removed from both oxygen and nitrogen in its effects upon the ignited wire, that in order more accurately to ascertain the relative position of the latter two gases, I made a few further experiments on them as contrasted with each other, and not with hydrogen. I first repeated my former experiment on these two gases, varying it only by changing the circumstances in the manner suggested by the present experiments, which on account of the vessel containing the wire being immersed in a given quantity of water, instead of being exposed to the external atmosphere, would occasion greater equality in the surrounding cooling effects, and would give me the opportunity of combining both methods in one experiment.

I filled both tubes A and B with oxygen, and included a voltameter in the circuit; in two minutes 3·43 cubic inches of hydrogen were evolved in the voltameter, and the thermometer in each cell had risen from 60° to 63°. A similar experiment with nitrogen gave in two minutes 3·4 cubic inches of hydrogen, and the thermometer rose from 60° to 63°.

This experiment accords with my previous one as to the voltameter test, but indicates no difference in oxygen and nitrogen with the thermometer test; I therefore in the following three experiments associated nitrogen with oxygen in the apparatus, fig. 1. All things being disposed as with the experiments on hydrogen associated with other gases, in five minutes the thermometer rose—

	In the oxygen.	In the associated nitrogen.
Exp. 1st.	From 60° to 71°·5.	From 60° to 73°.
2nd.	60° to 77°.	60° to 76°.
3rd.	60° to 75°.	60° to 76°.
Mean . .	60° to 74°·5.	60° to 75°.

The battery had increased somewhat in power after the first experiment, but as both wires formed part of the same circuit in each experiment, the variations in battery power do not affect the comparative results. The second experiment gives a variation in the position of oxygen and nitrogen with reference to the first and third experiments, but the gases so nearly approach in their cooling effects, that these slight differences are not much to be relied upon; however I applied a further

test. I associated in turn oxygen and nitrogen with carbonic acid; the following were the results. In five minutes the thermometer rose—

	In oxygen.	In carbonic acid.
Exp. 1st.	From 60° to 75°.	From 60° to 75°.
2nd.	60° to 76°.	60° to 75°.

	In nitrogen.	In carbonic acid.
Exp. 1st.	From 60° to 74°.	From 60° to 73°.
2nd	60° to 73°.	60° to 72°·5.

The battery had in the last experiment a little decreased in power; the oxygen and nitrogen both produced a less cooling effect than the carbonic acid, but the oxygen came nearer to it than the nitrogen, thus according with the previous experiments. Upon the whole it would appear that oxygen produces a somewhat greater cooling effect on the ignited wire than nitrogen, but these gases may, for the purposes of this paper, be fairly regarded as equal. Atmospheric air produces a similar effect to oxygen and nitrogen separately, though I am inclined to think that a slight chemical change takes place when atmospheric air is exposed to the ignited wire, and that nitrous acid is formed; for if litmus paper be held over a voltaically ignited platinum wire in the air, a slight but very perceptible tinge of red marks the portion of it immediately over the wire.

With the view of ascertaining whether the specific heat of the surrounding media were the cause of the phenomenon, I proceeded to try the effect of the wire carrying a voltaic current on different liquids; all things being disposed as in the previous experiments, and three ounces of water being associated respectively with the same quantity of the following liquids. The thermometer rose in five minutes—

In water, from 60° to 70°·3.		In spirit of turpentine.	60° to 88°.
In water, from 60° to 70°·3.		In sulphuret of carbon	60° to 87°·1.
In water, from 60° to 69°.		In olive oil	60° to 85°.
In water, from 60° to 70°·1.		In naphtha	60° to 78°·8.
In water, from 60° to 70°·5.		In alcohol sp. gr. 0·84	60° to 77°.
In water, from 60° to 68°·5.		In ether	60° to 76°·1.

I do not much rely on the last experiment,—the battery was in more feeble action; and though each of the above results is the mean of three experiments, yet the variations in the results of the different experiments with ether being considerable (while in the others they were very trifling), lead me to place no great dependence on it. The rapidity of evaporation and the readiness of ebullition of the ether require that a larger quantity should be used; but as this for the purpose of comparison would have required all the experiments to be repeated with different quantities

of liquid, I have not thought it worth while to go through the series a second time. It will be observed, that the effects with the above liquids are by no means in direct relation with their respective specific heats; but in order to bring the results of the experiments with liquids into comparison with those with gases, I now associated a gas with a liquid, viz. hydrogen with water. All things being disposed as before, the tube A was filled with hydrogen gas, the tube B with water, both being immersed in three ounces of water. The thermometer rose in five minutes—

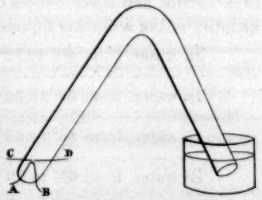
In hydrogen.	In water.
From 60° to 75°·5.	From 60° to 72°

This experiment of itself conclusively negatives the possibility of specific heat alone accounting for the phenomenon under consideration; and though, doubtless, specific heat must have some influence on the cooling effects of different gases and liquids, yet in the former it is apparently of very trifling import in comparison with the real physical cause of the differences, whatever that may be.

Supposing, as is stated by FARADAY*, that gases possess feeble conducting powers for voltaic electricity, and supposing hydrogen, from its close analogy in chemical character to the metals, to possess a greater conducting power than the other gases, this would account for its peculiar effect on the ignited wire, as a certain portion of the current, instead of forcing its way through the wire, would be carried off by the surrounding gas. In order to ascertain this I arranged the following experiments.

1st. Into the closed end of a bent tube, fig. 2, a loop of platinum wire, A B, and two separate platinum wires C D, were hermetically sealed, the extremities of the latter being approximated as closely as possible, and the interval between them being close to and immediately over the apex of the loop. The tube was filled with hydrogen, and the wire A B connected with a voltaic battery of sufficient power to raise it to as high a degree of ignition as it would bear without fusion; C and D were now connected with the poles of another battery, a delicate galvanometer being interposed in the circuit. Not the slightest effect on the galvanometer needle could be detected, and a similar negative effect took place when the tube was filled with atmospheric air.

Fig. 2.



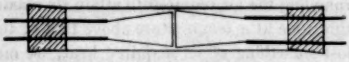
2nd. Parallel portions of platinum wire were now arranged in close proximity (see fig. 3.), and so that each might be ignited to a full incandescence by separate insulated batteries. When surrounded by atmospheres, both of atmospheric air and of hydrogen and fully ignited, not the slightest conduction could be detected, across the interval between the wires, with ten cells of the nitric acid battery, and being enabled

* Experimental Researches, §§ 272, 441 and 444.

by the kindness of Mr. Gassiot to repeat this experiment with his battery of 500 well-insulated cells of the nitric acid combination, air did not conduct when the ignited wires were approximated to the $\frac{1}{10}$th of an inch; on approaching them nearer they came within striking distance, were instantly fused, and the galvanometer needle, which had up to this time been perfectly stationary, was whirled rapidly round.

I think I am entitled to conclude from this, that we have no experimental evidence that matter in the gaseous state conducts voltaic electricity; probably gases do not conduct Franklinic electricity, as the experiments which would seem *primâ facie* to lead to that conclusion, are explicable as resulting from the disruptive discharge.

Fig. 3.

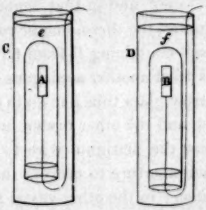


In Faraday's experiment two wires were approximated in the flame of a spirit-lamp, and a slight conduction across the interval in the flame was observed. This conduction might have been due to certain unconsumed particles of carbon existing in the flame, or possibly to the flame itself; according to Dr. Andrews, flame, even that of pure hydrogen gas, conducts voltaic electricity*.

I now endeavoured to ascertain whether any specific inductive effect of the hydrogen might have an influence: parallel wires of platinum and parallel coiled copper wires were placed in atmospheres of hydrogen and of atmospheric air, one of which parallel wires conveyed the current, and the other wire was connected with a delicate galvanometer. I could detect no difference in the arcs of deflection of the needle at the instant of meeting or breaking contact, whether the wires were in atmospheres of hydrogen or of atmospheric air; nor when parallel platinum wires with their surrounding atmospheres of gas were immersed in a given quantity of water, could I detect any difference in the resulting heat, whether the current passed in the same or in a different direction through each wire.

My next object was to ascertain whether, in cases of ordinary ignition, the same apparent annihilation of heat took place in hydrogen gas as with voltaic ignition. Two iron cylinders A B, fig. 4, each weighing 390 grains, were attached to long iron wires bent back in the form shown in the figure. The cylinders were placed together in a crucible of fine sand, and the whole heated to an uniform white heat. The cylinders were now taken out of the sand, placed at the surface of equal portions of water in the vessels C and D; two inverted tubes *e*, *f*, the one of hydrogen, the other of atmospheric air, were placed over them, and the whole quickly immersed in the water, and retained by a little contrivance, which I need not particularize, in the position shown in the figure. The

Fig. 4.



* Philosophical Magazine, vol. ix. p. 176.

temperature of the water at the commencement of the experiment was 60° Fahr. In four minutes the water surrounding the hydrogen had risen to 94°, and became stationary there, while that surrounding the air had only reached 87°; in ten minutes the water surrounding the hydrogen had sunk to 92°·5, while that surrounding the air had risen to 93°, which was the highest temperature it reached; thus the respective maxima were 94° and 93°; but considering the greater time which the water surrounding the air required to attain its maximum temperature, and that being during this time at a temperature above that of the surrounding atmosphere, it must have lost something of its acquired heat, we may fairly consider the maxima to be the same, and that the difference of effect in the two gases had reference solely to the time occupied in the transference of the heat. In a second experiment the results were similar, the maximum being in this experiment 92·5 in hydrogen, and 91 in air*.

As far as ordinary ignition is concerned, hydrogen has been shown by the experiments of Leslie and Davy to produce a more rapid cooling effect than air; and the above experiment having shown that it does not alter or convert into any other force the actual amount of heat given off, my next step was to inquire whether this rapidity of cooling effect of the hydrogen would account for the effects observed with voltaic ignition. Although the two classes of effects were apparently very different, it might be that the improved power of conduction arising from the rapid cooling effect of the hydrogen might, by enabling the current to pass more readily, carry off the force in the form of electricity, which if the wire offered more resistance (as it would when more highly ignited) would be developed in the form of heat. By employing the same medium, but impeding the circulation of the heated currents in one case, while their circulation was free in the other, some light might be expected to be thrown on the inverse relation of the conducting power to the heat developed. The following experiment was therefore tried.

In the apparatus represented in fig. 1, tube A was uncorked, so as to allow free passage for the water, while tube B was filled up with fine sand soaked with water, and then corked at both ends; the current was passed and the following was the result. In the vessel containing tube A, the thermometer rose in five minutes from 52° to 60°, and in that containing tube B from 52° to 60° also; during a second five minutes, the thermometer rose in the vessel containing A from 60° to 67°, and in the vessel containing B from 60° to 67° also.

I tried another analogous experiment: a coil of platinum wire was placed in a very narrow glass tube one-sixth of an inch diameter; this was hermetically sealed at one end, and the other drawn into a very narrow aperture, little more than sufficient to allow the platinum wire to pass, and filled with water (it was necessary to leave a small aperture to prevent the bursting of the tube by the expansion of the heated water); in the other vessel a similar coil of platinum wire was placed, but without

* Iron wire produces a similar effect to platinum wire in the voltaic experiments.

any glass tube at all. The circuit having been completed as before, the thermometer rose in five minutes—

In the water without the tube, from . . . . . 60° to 87°.
In the water containing the tube, from . . . . . 60° to 86°.

Here the difference, slight as it was, was against what theory would have led one to anticipate; the exact equality however of the previous experiment, and the close approximation of the results in this one, afford no conclusive information as to the point under consideration, though the negative result rather tends against the view which would assimilate the effects of voltaic to those of ordinary ignition.

As another method of attaining the object before mentioned, viz. the inverse relation of the conducting power of the wire to the heat developed in it, I tried the following experiment. A platinum wire of one foot long and $\frac{1}{50}$th of an inch diameter was ignited in air by ten cells of the battery, a voltameter being included in the circuit; the amount of hydrogen given off by the voltameter was one cubic inch in forty-four seconds: half the wire was now immersed in water of the temperature of 60° Fahr.; by this means the intensity of ignition of the other half was notably increased; the voltameter now yielded one cubic inch in forty seconds: two-thirds of the wire immersed, gave one cubic inch in thirty-seven seconds; and five-sixths immersed, gave one cubic inch in thirty-five seconds. The heat of the portion of wire not immersed in water had in the last experiment nearly reached the point of fusion of the platinum. By this result it appears that the increased resistance to conduction of the ignited portion is not equal to the increased conducting power of the cooled portion of the same wire.

With a view of seeing how far the cooling effect upon the ignited wire might be due to the greater or less fluency or mobility of the particles of the different media surrounding it, I have looked into the papers of Faraday* and of Graham†. In the experiments of the former, it appears that the escape of different gases at a certain pressure through capillary tubes, or the velocities of revolution of vanes or floats surrounded by different gases, was in some inverse ratio to the density of such gases; and the experiments of the latter show that the effusion or escape of gases through a minute aperture in a plate, takes place with velocities inversely as the square root of their specific gravities. In Graham's experiments, however, when the escape took place through capillary tubes, the results seemed subject to no ascertained law, though the compounds of carbon with hydrogen passed through with greater facility than other gases.

The cooling effects of gases on the ignited wire are decidedly not in any ratio with their specific gravities; thus, carbonic acid on the one hand, and hydrogen on the other, produce greater cooling effects than atmospheric air; and olefiant gas, which closely approximates air, and is far removed from hydrogen in specific gravity, much more nearly approximates hydrogen, and is far removed from air in its cooling effect.

* Quarterly Journal of Science, vol. iii. p. 354. † Philosophical Transactions, 1846, p. 573.

Upon the whole, we may conclude, from the experiments detailed in this paper, that the cooling effect of different gases, or rather the difference in the cooling effect of hydrogen and its compounds from that of other gases, is not due to differences of specific heat; it is not due to differences of specific gravity; it is not due to differences of conducting powers for electricity; it is not due to the character of hydrogen in relation to its transmission of sound, noticed by Leslie, for reasons which I have before given*; it is not due to the same physical characters of mobility which occasion one gas to escape from a small aperture with greater facility than another; but it may be, and probably is, affected by the mobile or vibratory character of the particles by which heat is more rapidly abstracted. I at one time thought that the effect might have relation to the combustible character of the gas, and that the electro-negative gases were in respect to it contra-distinguished from the electro-positive or neutral gases, but the experience I have obtained from the experiments detailed here induces me to abandon that supposition.

I incline to think, that, although influenced by the fluency of the gas, the phenomenon is mainly due to a molecular action at the surfaces of the ignited body and of the gas. We know that in the recognised effects of radiant heat, the physical state of the surface of the radiating or absorbing body exercises a most important influence on the relative velocities of radiation or absorption; thus, black and white surfaces are, as every one knows, strikingly contra-distinguished in this respect: why may not the surface of the gaseous medium contiguous to the radiating substance exercise a reciprocal influence? why may not the surface of hydrogen be as black, and that of nitrogen as white to the ignited wire? This notion seems to me the more worthy of consideration as it may establish a link of continuity between the cooling effects of different gaseous media and the mysterious effects of surface in catalytic combinations and decompositions by solids such as platinum. Epipolic actions will, I feel convinced, gradually assume a much more important place in physics than they have hitherto done; and the further development of them appears to me the most probable guide to the connection by definite conceptions of physical and chemical actions.

The difference of the cooling effect of hydrogen, and of those of its compounds, where it is not neutralized by a powerful electro-negative gas, from all other gases, is perhaps the most striking peculiarity of the phenomena I have described. The differences of effect of all gases other than hydrogen and such compounds are quite insignificant when compared with the differences between the hydrogenous and the other gases. There are some phenomena which I have before observed, and which were, at the time I noticed them, inexplicable to me; but they now appear dependent on this physical peculiarity of hydrogen. Thus, if a jet of oxygen gas be kindled in an atmosphere of carburetted hydrogen, the flame is smaller than when the converse effect takes place. The voltaic arc between metallic terminals is also much

* Philosophical Transactions, 1847.

smaller in hydrogen gas than in nitrogen, though both these gases are incapable of combining with the terminals; indeed to obtain an arc at all in hydrogen is scarcely practicable.

Davy has, in his Researches on Flame, given several experiments which are similarly explicable; but though noting the results, he nowhere, as far as I am aware, attributes them to any specific peculiarity of hydrogen.

Of the phenomenon which I have examined in this paper, I first published an account in connection with some experiments on the application of voltaic ignition to lighting mines, and it does not appear impossible that the experiments now detailed may ultimately find some beneficial application in solving the problem of a safety-light for mines. A light which is just able to support itself under the cooling effect of ordinary atmospheric air would be extinguished by air mixed with hydrogenous gas.

I am far from pretending to have devised any means of fulfilling these conditions, and yet supplying an efficient light; I merely throw it out as a suggestion for consideration, knowing that there are no additions to our knowledge which are not ultimately valuable in their practical application; and that a suggestion, however vague,—a new point to those whose minds may be occupied with the subject, may lead them to results which he who makes the suggestion is unable to attain.

P.S. Since this paper was communicated I have received a paper from Dr. Andrews of Belfast, who published as early as 1840, in the Proceedings of the Royal Irish Academy, experiments similar to those of mine first published in 1845. My experiments were made in the same year as those of Dr. Andrews, but as I withheld their publication, Dr. Andrews is fully entitled to priority. Had I known of his experiments earlier, I should have recited them in the first part of this paper.

VI. *On the Spontaneous Electrical Currents observed in the Wires of the Electric Telegraph.* *By* W. H. Barlow, *Esq.*, *M. Inst. C.E.*

Communicated by Peter Barlow, *Esq.*, *F.R.S.*

Received May 15,—Read May 25, 1848.

THE observations described in the following pages were undertaken in consequence of certain spontaneous deflections having been noticed in the needles of the electric telegraph on the Midland Railway, the erection of which was carried out under my superintendence as the Company's engineer.

The telegraph is on the principle patented by Professor Wheatstone and Mr. Cooke, and the signals are given by deflecting a magnetic needle suspended in a coil of fine wire, to the right or left, by means of a galvanic battery.

Each wire has an earth connection at its two extremities, and when a current is made to pass along the wire by means of the galvanic battery, it returns by the conducting power of the earth.

When the telegraph instruments are not working, the batteries are put out of circuit, and the wires remain with a simple earth connection at both extremities.

It was in this condition of the wires that spontaneous currents were observed to arise in them, producing occasionally large deflections in the needles. These deflections were sometimes to the right and sometimes to the left; at times they changed rapidly from right to left, at others they continued in one direction for periods varying from a few minutes to one or more hours.

The system of telegraphs which centres at Derby, consists of four main lines, viz.—

1st. From Derby in a southerly direction to Rugby.

2nd. From Derby to Birmingham, which approaches a south-westerly direction.

3rd. From Derby in a northerly direction to Leeds.

4th. From Derby in a north-easterly direction to Lincoln.

When these four telegraphs were brought into operation, it was observed that the spontaneous deflections were almost invariably simultaneous on all the instruments, and that when in the Birmingham telegraph the deflection was such as to indicate that the current was passing from the telegraph wires to the earth at Derby, the current in the Rugby wires was also passing towards the earth at Derby, while the two other telegraphs showed the current to be passing *from* the earth at Derby along the wires proceeding in a northerly and north-easterly direction. It was also found that when the current took a reversed direction in one telegraph, it was reversed in

all. There were some exceptions to this rule, but they were rare, and always of short duration.

The spontaneous deflections of the telegraph needles had been observed on other lines of railway as well as on the Midland, and they had been attributed to atmospheric electricity passing by the wire through the coil to the earth, or *vice versâ* from the earth to the atmosphere. This supposition was apparently strengthened, because during thunder-storms it has frequently occurred that the wires in the coils have been fused, the poles of the needles reversed, or the needles de-magnetized; but it is difficult to conceive any effect of atmospheric electricity that would account for the relative positions of the needles of the telegraphs proceeding from Derby northwards as compared with those proceeding southwards*.

My attention was strongly drawn to the subject by the constancy of these effects, when a circumstance occurred which imparted a new interest to the inquiry. On the evening of the 19th of March, 1847, a brilliant aurora was seen, and during the whole time of its remaining visible, strong alternating deflections occurred on all the instruments. Similar effects were observed also on the telegraphs on several other lines of railway.

Regarding with much interest these effects, which appeared to open a new field for investigation, I determined on making a systematic set of observations on the subject.

Each of the lines of telegraph centring at Derby consists of several wires; from Derby to Birmingham, and Derby to Rugby, there are five wires. From Derby to Lincoln there are three wires, and from Derby northwards there are seven wires.

At the time of commencing these experiments only three wires had been put in operation for telegraph business. There were two spare wires of the railway telegraph from Rugby and Derby, and thence to Leeds, unoccupied, and two others from Birmingham to Derby and from Derby to Normanton belonging to the Telegraph Company, and intended to form a portion of the commercial telegraph, which were also at liberty.

I applied to Mr. Hatcher, the engineer of the Telegraph Company, for permission to make use of their spare wires in the proposed experiments, which was freely accorded; and I am much indebted to this gentleman and to Mr. Culley, under whose management the Midland districts is placed, for the valuable assistance and information they have afforded me in this inquiry.

My first object was to make two delicate galvanometers, which was readily accomplished by making use of the "*detectors*" employed in ascertaining any defect in the insulation of the wires.

These instruments are similar in principle to those employed in working the telegraph, having a coil of fine wire about 1000 feet in length, in which an astatic needle

* Since this was written I have received a communication from Mr. Culley, in which he points out an important distinction between the effects of lightning and the aurora on the instruments of the telegraph needles, which I beg to add as a postscript to this paper.

is suspended, of which the lower end is made to preponderate, so that the natural position of the needle is vertical. The only alteration made to adapt this instrument to the purpose was supporting the needles on knife edges instead of circular bearings, and diminishing the gravitating preponderance of the lower end of the needles, which alterations increased the sensitiveness in a high degree.

The preliminary experiments were directed towards ascertaining whether the deflections were attributable to the electricity passing from the atmosphere along the wire to the earth. The observations were frequently repeated on wires from forty to fifty miles in length, and their results may be briefly stated as follows:—

Wires insulated from the earth throughout their entire length produced *no* deflection in either instruments.

Wires having an earth connection only at one extremity produced *no* deflection.

A complete circuit made by uniting both extremities of two wires, each forty-one miles long, and insulated from the earth throughout their length, produced *no* deflection.

But in every case deflections were obtained from a wire having an earth connection at both extremities.

Two wires having earth connections at both extremities produced a larger deflection than one wire.

A later experiment on the same subject showed that a wire having an earth connection at one extremity, and another earth connection near the middle of its length, gave a deflection on the part of the wire between the two earth connections, but none on the part beyond.

In watching the operations of the galvanometers when in circuit with a wire having two earth connections, it was observed that the needle was rarely found to remain in the same position many minutes, large variations taking place sometimes in a few seconds, and it became interesting to ascertain if these changes coincided at both ends of the wire.

In order to submit this to experiment, simultaneous observations were made at intervals of five minutes for twenty-four hours on two galvanometers, one at Derby and the other at Birmingham, each connected to the same wire.

Mr. Culley took the observations at Birmingham, while I took those at Derby, each being assisted by an intelligent telegraph clerk. In addition to the galvanometers, the wet and dry thermometers were also noted at every observation.

The results of these observations are given in diagram No. 1, and allowing for the difference in delicacy of the two galvanometers, there is enough to show that the currents were simultaneous in all their changes; it was also evident from this experiment that the direction of the current was the same at both extremities of the wire.

This fact, together with those previously mentioned, indicates that the currents which produce deflections do not arise from the transit of electricity between the atmosphere and the earth, but that from whatever cause the currents originate, they

travel along the wires from one earth connection to the other, alternating first in one direction and then in the other.

In examining the results obtained from this experiment, there appeared a general movement of the needle to the right from the commencement of the observations in the morning until midnight, then changing over to the left until nine or ten o'clock in the morning, when it again passed to the right, large and rapidly alternating deflections having occurred during the night, the effects of which were visible on the ordinary telegraph instruments. The general direction of the needle, however, independently of these irregular influences, appearing to exhibit some regularity, I followed up the experiments with the galvanometer at Derby, and found that a similar motion of the needle occurred daily.

This discovery led me to establish a series of observations for fourteen days and nights, on two wires simultaneously, one from Derby to Birmingham, and the other from Derby to Rugby, the position of the needle being recorded every five minutes, day and night. The mean position of the needles during each hour, as obtained from these observations, is given in Tables Nos. I. and II., and the mean result for each week is given in the right-hand column.

The path described by the two needles during the week, ending May 29, 1847, is also exhibited in the diagram No. 2.

In order to explain the directions in which the currents traversed the wires in these experiments, it is necessary to state that the two extremities of the coil in the galvanometers are attached to two brass screw pegs, technically called "terminals," which stand up on the top of the case of the galvanometer, one on the left hand and the other on the right, and the coil was so arranged that when the copper pole of a battery was connected with the *left* hand terminal, and the zinc pole with the right, the deflection (which in all cases refers to the upper end of the needle) was to the left; and assuming that the current flows from the copper to the zinc pole, a deflection to the left in these observations indicates a current flowing along the wire towards Derby, and a right-hand deflection shows the current to be flowing from Derby to the extremity of the wire.

In addition to the above-mentioned experiments, simultaneous observations were made with galvanometers on the wires proceeding from Derby northward and southward, the results of which showed that the currents producing the regular diurnal deflections followed the same law as to their relative directions in the four different lines of telegraph centring at Derby, as that which had been observed on the telegraph instruments during periods of the large spontaneous deflections.

The broad feature elicited by these observations may therefore be stated to be,—

1st. That the path described by the needle consisted of a regular diurnal motion, subject to disturbances of greater or less magnitude.

2nd. That this motion is due to electric currents passing from the northern to the southern extremities of the telegraph wires, and returning in the opposite direction.

3rd. That exclusive of the irregular disturbances the currents flowed in a southerly direction from about eight or nine A.M. until the evening, and in a northerly direction during the remainder of the twenty-four hours.

The next experiments were made with a view to ascertain if any immediate relation existed between the motion of the galvanometers and the daily variation of the horizontal magnetic needle.

For this purpose I caused a temporary observatory to be erected in my garden at Derby (about a mile from the railway station, where the galvanometer experiments were made), and furnished it with a very delicate declinometer.

On making observations with the two instruments, it became evident that, although generally that part of the day in which the currents flow southwards (that is, from eight or nine A.M. until the evening) the variation of the horizontal magnetic needle is westerly, and that during the night and early part of the morning (at which time the currents travel northwards) the variation is easterly; yet simultaneous observations showed no similarity in the path described by the magnetic needle and the galvanometer.

It had however been mentioned by Colonel SABINE, when my former paper on this subject was read, which described large deflections having occurred on the evening of the 19th of March, 1847, that unusual disturbances had been observed at the same time in the magnetic needle not only in England but abroad; I therefore waited for an opportunity to repeat the experiments with the declinometer at a time when the telegraph needles were unusually deflected.

On the 24th of September 1847, I was enabled partially to carry out this intention; and on the 27th I obtained a set of simultaneous observations on the galvanometer and the magnetic needle, the galvanometer being attached to a wire having its earth connections at Derby and Rugby.

These observations show unusual disturbances on both instruments on the days mentioned, particularly on the 24th, when it was excessive.

From communications I have been favoured with, it appears that the deflections of the telegraph needles on the 24th of September were general throughout the kingdom. They were observed on the South Devon line and in Scotland, as well as on all the lines in this part of the country; and it is worthy of remark, that all the reports of the telegraph clerks agree nearly in the time of the commencement of the disturbance, the earliest time stated being 11·35 A.M., and the latest noon.

The 23rd of October was another day of strong deflections, and a partial register was kept of them in London by Mr. HATCHER. They were equally strong on the Midland line, and I have accounts of them as far as Newcastle. The 24th being Sunday, there was no register kept; but they continued on the 25th, and were again registered by Mr. HATCHER in London, who has favoured me with his observations; but beyond the fact of the unusual disturbance, they throw no additional light on this subject.

I have no magnetic observations on the 23rd, 24th, or 25th of October, but I have since learnt that an unusual magnetic disturbance occurred on these days, and there appears no doubt of the coincidence of these great disturbances in both instruments.

On the three occasions mentioned, namely, the 19th of March, the 24th and 25th of September, and the 23rd, 24th and 25th of October, aurora was visible; and in every case which has come under my observation, the telegraph needles have been deflected whenever aurora has been visible.

It only remains now to describe the experiments made to ascertain the line of direction in which the currents alternate, and it will serve to render this part of the subject more clear, to state in this place that, from numerous experiments, it appears that from whatever cause the currents are produced, the direction of the current at a given time in any wire depends on the relative positions of the earth connections, if the insulation is good, however circuitous may be the route of the wire itself. For example, the telegraph from Derby to Rugby, forty-nine miles in length, proceeds for ten miles about S.E. by E. to Kegworth; then for nineteen miles it takes a S.E. direction to Leicester, and the remaining twenty miles to Rugby is about S.S.W.

Having one earth connection at Derby, if the other be made at Rugby, the bearing of which place from Derby is S. 15° E.; the deflections accord with those of the wire to Birmingham, bearing S. 29° W.; but if the earth connection of the Rugby wire be changed from Rugby to Kegworth, the bearing of which from Derby is S. 62° E., the deflections produced are in the contrary direction to those of the Birmingham wire.

I do not consider that this fact in itself proves that the currents are generated in the earth, for they might arise from other causes, and yet exhibit the same result. I only mention the fact in this place to facilitate the consideration of the direction in which the currents alternate, and to indicate that when the direction of the current between any two places is described, it is not meant that the wire is laid in a direct line between the two points, but that the earth connections are so placed.

Referring the direction of deflection in every case to those produced by the Birmingham wire, and denoting those which accord with it by the sign +, and those which exhibit a contrary deflection −, the results of the experiments on direction were as follows:—

Derby to Willington, bearing	S.W.	+
Derby to Birmingham, bearing	S. 29° W.	+
Derby to Rugby, bearing	S. 15° E.	+
Derby to Leicester, bearing	S. 38° E.	+ −
Derby to Loughborough, bearing . . .	S. 50° E.	doubtful.
Derby to Kegworth, bearing	S. 62° E.	−
Derby to Nottingham, bearing	N. 80° E.	−
Derby to Lincoln, bearing	N. 60° E.	−
Derby to Chesterfield, bearing	N. 5° E.	−
Derby to Normanton, bearing	N. 3° W.	−

The observations were made with galvanometers on two or more wires simultaneously, and the motions of the needles observed for several hours. It frequently happens that, from some difference in the earth connection, there is a slight permanent action, so that in making these observations it is not simply the position but the motion of the needles that distinguishes the direction of the current. If on trying two wires, one causes the galvanometer needle to move from right to left, when the other moves from left to right, the currents are in opposite directions.

Mr. CULLEY has favoured me with another set of observations obtained from the telegraph instruments at Normanton, during large deflections.

Normanton is a central station from which seven telegraphs branch off. Each telegraph has an earth connection at Normanton, and the other extremities of the telegraphs are connected with the earth at Rugby, Derby, Manchester, Leeds, Newcastle, York and Hull.

Calling those telegraphs in which the deflections accord with that to Manchester +, the results are as follows:—

From Normanton to Rugby. . .	S. 5° E. +
From Normanton to Derby . . .	S. 2° W. +
From Normanton to Manchester .	S. 65° W. +
From Normanton to Leeds . . .	N. 35° W. doubtful —, very small deflections.
From Normanton to Newcastle .	N. 8° W. doubtful, generally —.
From Normanton to York . . .	N. 40° E. —
From Normanton to Hull . . .	N. 87° E. —

The general result derived from these two sets of experiments may be stated as follows:—Taking one earth connection as a point of reference when the bearing of the other earth connection lies between S. and W., or between N. and E., the action is strong and decided, the one being + and the other —.

In the experiments made from Derby as a central point, the action of the current is reversed when the earth connection is changed from S. 15° E. to S. 62° E. I have tried numerous experiments between these two directions, and there does not appear to be any line in which all action ceases, but in approaching the S.E. direction the motion of the needles becomes undefined.

The direction in which the currents travel being supposed to be at right angles to that in which the reversed action takes place, will be between S. 28° W. and S. 75° W., and apparently strongest when the earth connections are about N.E. and S.W.

As the fact above mentioned, namely, that the direction of the current in any wire at a given time depends on the relative positions of the earth connections, and not on the direction of the wire itself, is of great interest, I have recently repeated some of the observations on this subject, the results of which are given in diagram No. 3.

In these observations, a spare wire from Derby to Willington, length 6½ miles, bearing S.W., was used as a standard of comparison, and simultaneous observations were made on it, and on a wire from Derby to Rugby, varying the position of one of the earth connections of the Rugby wire as described below.

First Day's Observations, May 1st, 1848.

The positions of the earth connection were,—

No. 1 wire. Derby and Willington, bearing S.W.
No. 2 wire. Derby and Rugby, bearing S. 15° E.

The path described by the two galvanometer needles is shown in figs. 1 and 2, and it will be seen that there is an obvious similitude throughout all their movements.

Second Day's Observations, May 3rd, 1848.

The position of the earth connections were,—

No. 1 wire. Derby and Willington, bearing S.W.
No. 2 wire. Derby and Leicester, bearing S. 38° E.

The movement of the galvanometers is shown at figs. 3 and 4. There is a partial similarity in the forenoon, but none afterwards.

Third Day's Observations, May 5th, 1848.

Earth connections the same as in the last experiment.

The paths of the galvanometers is shown at figs. 5 and 6. There is no similarity in the movement of the needles, excepting that the general march of both needles is from the left in the morning, towards the right in the afternoon.

Fourth Day's Observations, May 8th, 1848.

The position of the earth connections were,—

No. 1 wire. Derby and Willington, bearing S.W.
No. 2 wire. Derby and Kegworth, bearing S. 62° E.

The movement of the needles is shown at figs. 7 and 8.

In this case we have a contrary direction of the current clearly marked and rendered more evident by the larger deflections which occurred on this day.

These experiments are satisfactory as verifying the former observations made on this subject from time to time during the last twelve months.

It should be mentioned that no part of the Rugby wire was *disconnected* in these experiments, but that the whole length of wire was in action, and therefore exposed to the same influences from atmospheric currents, induction, or thermo-electric action. There was no alteration whatever made in the wire excepting the *change of position of one of its earth connections*; and consequently the reversing of the direction of the current in the wire, as compared with that of the Willington wire, cannot be attributed to any other cause.

The question naturally presents itself, from whence do these currents arise?

On this subject an important fact was ascertained during the large deflections which occurred in September and October, namely, that spontaneous deflections of

precisely the same character as those described in the foregoing part of this paper, were found to take place on the short telegraph from the Electric Telegraph office in the Strand to the Nine Elms Station, the wires of which are *laid underground in tubes throughout their length.*

Taking this fact in connection with those before mentioned, viz. that no deflection is produced in a wire suspended throughout its length in the air, that no deflection is produced with a wire having only *one* earth connection, but that in every case deflections are exhibited in a wire having two earth connections, and that the direction of the current in the wire at any given time is dependent on the relative positions of the earth connections, the most probable explanation appears to be that the currents are terrestrial, of which a portion is conveyed along the wire, and rendered visible by the multiplying action of the coil of the galvanometer.

POSTSCRIPT.

(*Copy.*)

My dear Sir, Derby, May 8, 1848.

It has often occurred to me, that if the deflections were caused by atmospheric electricity, they should occur before and during storms. I have never observed this to have been the case; the needles are seldom moved by lightning, and if they are, it is in spasmodic twitchings, perfectly different to the most rapidly varying deflections; but the bells are generally rung if a storm occur at any point of their circuit: on the other hand, deflections, unless exceedingly powerful, do not ring the bells.

A marked difference is always observed in the effect on the bells, between lightning and deflections, the first causing them to ring only a second, the last for several minutes.

I have twice this winter foretold an aurora; the connection between this phenomenon and the deflections is indisputable.

In the great storm at Leeds, Huddersfield, and the neighbourhood, of a few Sundays since, I had four pair of needles demagnetised at Normanton, one at Skipton, and a discharge between the points of the conductors at Bradford. The wires were disconnected from the instruments for safety, and a discharge took place from the free ends; still the needles were not deflected in the least degree, either before or after the storm, nor at any time when the instruments were in circuit during its continuance.

I had an excellent opportunity at Normanton last Monday of testing the direction of the line of "no-action."

The Manchester and the Derby instruments were each strongly deflected. I opened the circuit on one needle from Manchester to Derby, leaving the other needles on

the earth at Normanton as usual. The deflections continued as before on the circuit from Derby to Normanton, and Manchester to Normanton, but disappeared on the Derby to Manchester circuit, these places lying nearly in the line of "no-action," as determined by your experiments.

I find the line from London to Derby but slightly affected, and that from Normanton to Leeds is almost entirely free.

I am, my dear Sir, yours very truly,

(*Signed*) R. S. CULLEY.

W. H. BARLOW, *Esq.*, *Derby*.

Table I.

Mean deflections in each hour exhibited by a galvanometer at Derby, in connection with a wire extending to Rugby, for fourteen days, commencing May 17, 1848. (The means are taken from twelve observations in each hour.)

Rugby Instrument.

Time.	May 17.		May 18.		May 19.		May 20.		May 21.		May 22.		May 23.		Mean.	
	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.
1 A.M.	...	...	...	2·42	3·92	...	...	...	...	2·71	4·12	...	0·14	...	0·61	
2	...	...	6·29	...	5·37	...	...	...	3·87	...	2·21	...	0·50	...	3·65	
3	...	...	...	4·50	5·87	...	...	...	...	1·42	3·87	...	0·71	...	0·91	
4	...	...	...	2·50	...	3·00	...	...	2·00	...	1·17	...	0·40	...	...	0·38
5	...	...	...	1·83	...	1·75	...	...	1·58	...	...	0·83	...	...	...	0·71
6	...	...	...	1·50	0·25	...	...	...	0·92	...	2·83	...	...	...	0·62	
7	...	...	0·45	...	2·25	...	...	...	...	0·21	2·00	...	...	...	1·12	
8	...	...	3·09	...	6·83	...	...	...	0·66	...	3·66	...	...	...	3·56	
9	...	...	5·66	...	2·17	...	...	...	1·87	...	...	0·87	...	...	2·21	
10	...	...	4·16	...	...	3·08	...	1·46	0·33	...	...	3·75	...	...	...	0·76
11	...	...	0·42	...	...	2·50	...	2·00	...	3·09	...	5·33	...	...	...	2·50
12 Noon.	...	...	...	3·17	...	5·08	...	3·25	...	1·62	...	5·08	...	...	...	3·64
1 P.M.	...	...	...	5·17	2·25	...	...	1·79	...	4·71	...	2·42	...	...	...	2·87
2	...	...	...	2·75	...	2·42	...	3·64	0·32	...	...	1·08	...	...	...	3·31
3	...	...	...	7·08	...	0·33	0·66	...	...	2·17	...	1·05	...	...	...	1·99
4	...	9·11	...	9·60	...	0·46	...	4·50	...	2·54	...	1·29	...	...	...	4·58
5	...	7·12	...	6·20	...	...	...	6·25	...	0·62	...	2·12	...	...	...	4·46
6	...	10·82	...	11·33	...	...	2·05	...	...	3·46	...	1·33	...	...	...	4·98
7	3·12	...	...	5·37	...	...	...	10·42	...	4·25	...	1·25	...	...	...	3·63
8	7·83	...	2·33	...	...	...	...	5·58	...	6·12	...	3·08	...	...	...	0·92
9	12·34	...	2·50	...	...	...	6·83	...	...	1·00	...	2·54	...	...	3·62	
10	12·12	...	4·58	...	...	...	...	4·25	...	5·71	...	2·00	...	...	9·5	
11	0·33	...	5·73	...	...	...	4·87	...	0·42	...	...	2·14	...	...	1·84	
12 Midnight.	3·29	...	6·46	...	...	...	6·04	...	4·79	...	...	0·00	...	...	4·12	

Time.	May 24.		May 25.		May 26.		May 27.		May 28.		May 29.		May 30.		Mean.	
	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.
1 A.M.	...	...	0·25	...	0·25	...	...	0·08	...	4·79	...	4·70	...	...	...	1·81
2	...	...	2·40	...	3·37	...	...	0·41	0·66	...	...	1·08	...	...	0·98	
3	...	...	1·50	...	6·41	...	0·29	...	3·04	...	2·41	...	...	...	2·73	
4	...	...	5·62	...	3·54	...	0·95	...	...	0·16	...	2·25	...	...	1·50	
5	...	...	6·87	...	3·41	...	4·16	...	...	2·75	...	2·83	...	...	1·77	
6	...	...	4·08	...	2·79	...	5·79	...	7·58	...	2·12	...	...	...	4·47	
7	...	...	2·62	...	3·45	...	5·58	...	12·41	...	1·20	...	...	...	5·05	
8	...	...	1·04	...	0·91	...	3·83	...	8·16	...	4·41	...	...	...	3·67	
9	...	...	zero.		...	1·08	2·37	...	4·33	...	6·95	...	...	...	2·51	
10	...	...	...	1·29	...	2·29	1·33	...	7·50	...	...	11·08	...	...	...	1·24
11	...	...	...	2·70	...	4·16	...	1·29	...	1·33	...	13·70	...	...	...	4·63
12 Noon.	...	...	...	3·66	...	4·62	...	3·63	...	6·83	...	10·70	...	...	...	5·88
1 P.M.	...	1·62	...	3·79	...	4·37	...	4·75	...	6·54	...	7·70	...	...	...	4·79
2	...	1·29	...	1·68	...	3·45	...	3·87	...	9·0	...	13·56	...	...	...	5·47
3	...	0·62	0·12	...	1·87	...	...	1·62	...	2·00	...	8·70	...	...	...	1·82
4	...	0·58	1·41	...	...	0·45	0·62	...	0·25	...	...	4·58	...	...	...	0·55
5	...	0·50	1·28	...	4·66	...	2·45	...	...	2·37	...	6·95	...	...	...	0·24
6	...	0·37	...	0·16	4·05	...	...	1·22	...	3·50	...	6·93	...	...	...	1·35
7	0·12	...	0·16	...	3·62	...	...	4·95	4·08	...	...	1·00	...	...	0·34	
8	...	0·04	...	0·20	2·54	...	...	4·50	4·41	...	...	0·95	...	...	0·21	
9	...	0·95	...	1·75	1·66	...	5·08	...	4·33	...	...	0·20	...	...	1·36	
10	...	2·37	...	0·29	1·95	...	7·87	...	2·75	...	...	0·70	...	...	1·53	
11	...	0·54	...	0·79	...	0·29	7·91	...	2·58	...	...	2·58	...	...	1·05	
12 Midnight.	...	0·29	3·50	...	0·08	...	1·56	...	1·12	...	...	4·37	...	...	0·26	

Table II.

Mean deflections in each hour exhibited by a galvanometer at Derby, in connection with a wire extending to Birmingham, for fourteen days, commencing May 17, 1848. (The means are taken from twelve observations in each hour.)

Birmingham Instrument.

Time.	May 17.		May 18.		May 19.		May 20.		May 21.		May 22.		May 23.		Mean.	
	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.
1 A.M.	...	...	...	3·21	2·00	...	15·92	...	...	1·80	13·24	...	4·12	...	5·04	
2	...	...	3·75	...	5·25	...	1·50	...	6·02	...	6·25	...	5·66	...	4·74	
3	...	...	...	11·37	14·50	...	...	5·66	...	0·50	10·75	...	5·16	...	2·15	
4	...	...	...	8·10	...	2·50	0·50	...	3·83	...	6·25	...	4·30	...	0·71	
5	...	...	...	7·04	...	9·25	...	7·10	8·25	...	2·25	...	...	...	...	2·58
6	...	...	...	4·29	4·75	5·00	...	...	1·85	...	8·16	...	...	...	1·19	
7	...	...	0·66	...	4·25	...	5·83	...	...	0·58	7·50	...	...	...	3·53	
8	...	...	10·66	...	15·16	...	8·60	...	3·50	...	12·22	...	...	...	10·03	
9	...	...	9·50	...	4·33	...	1·25	...	8·42	...	0·42	...	...	...	4·78	
10	...	...	3·66	...	...	5·60	0·92	...	6·75	...	...	12·33	...	...	...	1·32
11	...	...	...	4·83	...	4·21	...	5·83	...	11·92	...	18·70	...	...	...	9·10
12 Noon.	...	...	...	15·29	...	16·37	...	11·25	...	6·16	...	16·21	...	...	...	13·06
1 P.M.	...	...	...	15·79	...	6·50	...	7·92	...	18·88	...	10·41	...	...	...	11·90
2	...	...	...	18·92	...	11·80	...	15·21	...	3·66	...	8·10	...	...	...	11·54
3	...	...	...	19·42	...	7·12	...	5·00	...	13·54	...	5·88	...	...	...	10·19
4	...	22·00	...	22·85	...	14·46	...	14·75	...	12·10	...	8·66	...	...	...	15·80
5	...	19·85	...	3·10	...	19·00	...	12·17	...	3·50	...	12·08	...	...	...	11·61
6	...	22·21	...	18·04	...	17·46	0·01	...	...	10·42	...	5·73	...	...	...	12·31
7	2·54	...	...	10·12	...	15·71	...	11·12	...	17·33	...	7·00	...	...	...	9·79
8	13·83	...	1·66	...	...	10·33	...	8·87	...	16·08	...	11·62	...	...	...	5·23
9	20·08	...	2·65	...	...	0·01	12·83	...	...	0·25	...	9·42	...	...	4·31	
10	13·71	...	4·71	...	4·75	...	...	7·00	...	10·16	...	7·83	...	...	...	0·30
11	...	1·83	7·00	...	...	6·50	12·83	...	2·04	...	...	7·42	...	...	1·02	
12 Midnight.	2·16	...	4·10	...	24·42	...	10·30	...	10·83	...	3·04	...	...	...	9·14	

Time.	May 24.		May 25.		May 26.		May 27.		May 28.		May 29.		May 30.		Mean.	
	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.
1 A.M.	...	...	0·20	...	0·54	...	...	0·83	...	3·58	...	3·87	...	...	...	1·50
2	...	...	1·79	...	3·04	...	...	0·75	0·20	...	...	0·83	...	...	0·69	
3	...	...	0·75	...	6·08	...	zero.		1·50	...	...	1·16	...	...	1·43	
4	...	...	4·12	...	0·75	...	0·50	...	...	0·70	...	1·00	...	...	0·73	
5	...	...	4·29	...	1·62	...	1·79	...	...	3·50	...	2·04	...	...	0·43	
6	...	...	1·70	...	0·79	...	2·79	...	6·29	...	4·91	...	...	...	3·29	
7	...	...	0·62	...	1·54	...	3·04	...	10·79	...	6·91	...	...	...	4·58	
8	...	...	0·12	...	...	0·04	1·79	...	5·91	...	8·66	...	...	...	3·28	
9	...	...	zero.		...	1·04	0·75	...	3·58	...	8·25	...	...	...	2·30	
10	...	...	...	1·20	...	2·16	0·37	...	6·64	...	...	5·37	...	...	...	0·34
11	...	...	...	2·46	...	4·37	...	0·79	...	1·54	...	6·87	...	...	...	3·20
12 Noon.	...	...	...	3·79	...	5·08	...	4·18	...	5·83	...	4·50	...	...	...	4·67
1	...	4·66	...	4·33	...	5·62	...	5·33	...	6·04	...	2·41	...	...	...	4·73
2	...	3·71	...	2·16	...	5·33	...	4·62	...	10·00	...	6·00	...	...	...	5·30
3	...	0·87	...	1·02	...	4·58	...	2·41	...	3·35	...	3·95	...	...	...	2·69
4	...	0·37	...	0·25	...	2·54	...	1·25	...	2·45	...	1·25	...	...	...	1·35
5	...	0·10	...	0·28	2·33	...	...	0·25	...	4·83	...	3·54	...	...	...	1·11
6	...	1·04	...	2·00	2·25	...	...	3·86	...	7·35	...	2·50	...	...	...	2·41
7	...	1·04	...	1·62	0·29	...	...	8·29	1·66	...	2·66	...	...	...	...	1·06
8	...	0·83	...	1·37	0·29	...	...	7·16	1·08	...	2·25	...	...	...	...	0·95
9	...	1·71	...	2·70	...	0·08	2·83	...	1·70	...	2·62	...	...	...	...	
10	...	3·00	...	0·75	0·37	...	6·04	...	...	0·33	1·83	...	...	...	0·44	
11	...	0·70	...	1·29	...	1·37	7·87	...	1·91	...	1·33	...	...	...	0·69	
12 Midnight.	...	0·45	3·20	...	...	2·91	0·62	...	1·25	...	0·20	...	...	...	1·29	
															0·32	

VII. *On the Meteorology of the Lake District of Cumberland and Westmoreland; including the results of Experiments on the fall of Rain at various heights above the Earth's surface, up to* 3166 *feet above the mean sea level.*

By JOHN FLETCHER MILLER, *Esq.*

Communicated by Lieut.-Col. SABINE, *For. Sec. R.S.*

Received April 4,—Read May 18, 1848.

Introduction.

NEARLY four years have now been devoted to the investigation of the fall of rain in the lake districts of Cumberland and Westmoreland; and two complete years have elapsed since the experiments were commenced, with a view to ascertain the amount of rain deposited at great elevations above the sea, extending to the tops of our highest English mountains.

As the investigations proceeded, some remarkable results were elicited, which coming to the knowledge of the Royal Society early in last year (1847), the Council kindly expressed a wish to contribute, from the Donation Fund, the sum of twenty pounds towards the current expenses attending the inquiry. The donation was accompanied by a request, that as early as convenient after the close of the then current year, I would transmit to the Royal Society a *resumé* of all that I had done in this department of meteorology.

This *resumé* I have endeavoured to communicate to the Society in the annexed paper.

Whitehaven, March 1848. J. F. MILLER.

Table I.

Fall of Rain in the Lake District of Cumberland, &c., for Six Months, commencing the 1st day of July, and ending the 31st day of December 1844.

1844.	Whitehaven.	Keswick.	Ennerdale Lake, Bowness.	Stonywath, two miles west of the lake.	Lowes water Lake.	South end of Crummock Lake.	Gatesgarth.	Westmoreland,		Wastdale Head.
								Troutbeck near Kendal.	Grasmere.	
July.....	4·183	3·052	5·549	4·151	3·425	4·59	5·70	4·178	4·874	6·36
August	1·999	5·727	4·863	4·164	5·564	7·55	9·08	4·623	6·856	10·74
September ...	5·809	4·780	6·327	5·417	6·185	6·33	7·92	5·724	5·381	9·23
October	4·335	5·272	6·240	4·493	7·121	8·22	10·78	6·142	8·644	9·45
November ...	1·926	2·842	3·700	2·151	3·048	3·69	5·49	3·987	6·397	5·26
December ...	·309	·108	·790	·170	·307	·33	·47	1·022	845	·47
Total	18·561	21·781	27·469	20·546	25·650	30·71	39·44	25·676	32·997	41·51
Wet days.....	79	84	.....	.....	75	83	93	82	77	71

Table III.

Wet days in 1845.

1845.	Whitehaven.	The Plash.	Cockermouth.	Keswick.	Loweswater.	Crummock Lake.	Gatesgarth.	Wastdale Head.	Grasmere.	Langdale.	Troutbeck.	Seathwaite.
January	21	16	19	21	21	21	21	20	22	...	18	22
February.....	10	5	6	9	8	8	11	9	9	...	4	11
March.....	14	11	14	14	14	12	15	13	15	...	12	15
April	13	10	11	12	12	11	12	12	12	...	12	11
May	14	14	16	15	15	17	16	14	12	...	12	15
June	16	15	18	17	15	16	17	16	16	18	17	18
July.....	13	15	17	11	15	15	16	16	16	15	17	15
August	17	20	21	20	18	21	19	21	15	20	16	22
September ...	15	12	17	13	13	16	16	17	14	17	14	15
October	20	20	26	21	19	23	23	26	21	22	19	21
November ...	17	15	21	19	20	18	19	21	19	19	17	20
December ...	23	22	26	23	25	24	25	26	25	26	22	26
Days	193	175	212	195	195	202	210	211	196	137	180	211

Table II.

Synopsis of the Fall of Rain in the Lake Districts, &c. of Cumberland and Westmoreland, in the year 1845.

1845.	Whitehaven.		The Flosh, 2¼ miles south of Whitehaven.	Cockermouth.	Keswick.	Esmerdale Lake.	Loweswater Lake.	Crummock Lake.	Gatesgarth.	Wastdale Head.	Westmoreland.			Seathwaite, Borrowdale.	Prevailing winds.	
	6 feet above the ground.	St. James's Church Steeple, 78 feet above the street.									Troutbeck, 9 miles N.W. of Kendal.	Grasmere.	Langdale Head.		At the coast.	At Crummock Lake.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.		
January ...	4·541	2·767	4·88	3·195	6·426	7·560	7·100	9·53	13·78	11·00	7·031	15·023	.....	16·81	S.W.	S.W.
February...	2·830	2·025	2·76	1·045	1·004	2·160	1·897	1·70	2·61	3·40	1·986	2·252	.....	3·48	S.	S. var.
March.....	3·735	2·850	4·24	3·895	3·978	6·715	5·814	7·06	9·07	10·37	7·001	10·256	.....	13·21	N.E. & S.E.	E.
April	2·587	1·770	2·67	2·330	4·886	5·500	4·859	7·31	10·60	7·40	5·595	6·133	.....	10·57	N.E.	E.
May	1·480	·995	1·74	1·970	2·270	3·200	2·629	3·62	4·90	4·13	2·205	2·289	.....	4·57	N.E. & E.	E.
June	4·099	3·172	3·86	3·090	4·032	5·550	4·954	5·97	8·02	6·15	5·206	5·010	7·14	8·25	S.W.	N.W.
July	2·900	1·980	3·52	2·600	4·112	5·050	5·274	6·39	8·72	5·42	4·366	6·191	7·65	8·65	S.W. var.	S.W.
August ...	6·995	5·100	7·22	8·720	8·806	10·005	8·858	11·17	15·75	12·31	5·934	10·185	13·01	15·61	N.W. var.	N.E. var.
September	3·653	2·405	4·11	3·825	5·004	5·480	4·576	6·54	8·49	7·53	4·751	6·538	8·55	9·77	S.W. & N.W.	S.W.
October ...	6·744	4·853	7·35	6·595	5·948	8·745	7·446	8·20	10·88	12·35	8·130	11·785	13·41	15·17	N.W.	N.W.
November	4·022	2·350	4·40	3·095	7·140	6·740	6·966	8·50	13·48	12·31	8·545	21·732	18·84	20·84	S.W.	N.W. & S.W.
December	5·621	3·222	6·25	6·570	8·596	10·175	9·169	11·49	17·83	16·18	15·555	23·614	24·02	24·94	N.W.	N.W.
Inches.....	49·207	33·489	53·00	46·930	62·202	76·880	69·542	87·48	124·13	108·55	76·305	121·008	92·62 from June	151·87	S.W. var.	S.W. var.

The *greater portion* of the Tables for 1845, and the last six months of 1844, have appeared in the Edinburgh Philosophical Journal, but are added here for the sake of completeness.

Table IV.

Synopsis of the Fall of Rain in the Lake Districts of Cumberland and Westmoreland in the year 1846.

1846.	Whitehaven.			The Flosh, 3 miles south of Whitehaven.	Cockermouth.	Keswick, 250 feet above the sea.	Ennerdale.		Loweswater Lake, 326 feet above the sea.	Crummock Lake, 262 feet above the sea.	Gatesgarth, 3267 feet above the sea.	Eskdale Foot, height unknown.	Wastdale Head, 166 feet above the sea.	Westmoreland.			Seathwaite.		Prevailing winds.		
	High Street, 90 feet above the sea.	Round Close, 480 feet above the sea.	Saint James's Church Steeple, 78 feet above the street.				Bowness, 246 feet above the sea.	Vale of Gillerthwaite, 246 feet above the sea.						The How, Troutbeck.	Grasmere, 180 feet above the sea.	Langdale Head, 250·7 feet above the sea.	At 6 inches above the surface.	At 18 inch. above the surface.	At the coast, two daily observations.	At Crummock Lake, two daily observations.	At Seathwaite, one daily observation.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.			
January ...	4·604	.....	3·290	5·44	4·150	5·112	8·084	.....	7·062	8·98	12·81	.....	12·97	8·827	17·013	15·63	17·07	.....	S.W.	S.W.	S.W.
February...	2·007	.....	·960	2·17	2·445	4·840	4·116	.....	4·517	6·57	9·88	.....	6·60	5·786	10·917	12·25	11·51	.....	S. & S.W.	S.&N.W.var.	N.W.
March.....	4·460	.....	2·150	4·60	5·115	8·542	8·940	.....	8·675	11·94	15·07	.....	10·35	9·774	15·964	16·02	17·85	.....	S.W.	S.W.	S.W.
April	2·848	2·712	2·130	3·60	2·630	4·546	5·190	.....	4·395	5·52	7·11	4·38	6·59	4·790	6·968	6·24	7·70	.....	N.E.	E.	S.E.
May.....	2·317	2·437	1·623	2·64	1·680	1·724	3·130	3·48	2·505	3·62	3·69	3·15	3·65	3·134	2·059	4·13	4·40	.....	S.W.	W.	S.W.
June	2·311	2·103	1·680	2·38	3·840	4·710	5·020	5·76	5·010	5·70	5·85	3·91	5·33	3·742	5·916	5·83	6·41	6·46	S.W. var.	W. & S.W.	S.W.
July.....	9·061	8·626	6·995	8·68	10·190	9·320	13·480	15·20	13·430	15·21	19·51	9·12	16·82	10·040	13·909	16·32	20·80	21·63	S.W.	S.W.	S.W.
August ...	4·066	3·465	3·320	5·31	4·650	4·676	7·320	9·01	5·420	6·92	10·68	6·82	8·96	5·552	6·685	8·45	10·58	10·87	N.E. & N.W.	S.E.	S.E.
September	2·857	2·574	2·157	2·86	2·100	2·902	3·250	2·75	2·905	3·19	3·33	3·83	3·79	3·459	3·310	4·51	4·60	4·78	S.E. var.	S. to W.	W. var.
October...	7·982	7·802	6·115	9·54	8·940	12·248	14·240	15·78	13·490	16·22	18·78	10·62	15·75	12·014	16·320	21·25	25·43	26·22	S.W.	S.W.	S.E. & N.W.
November	4·671	4·587	3·760	4·90	4·080	6·472	7·100	7·90	7·745	8·70	10·15	6·36	8·59	7·539	7·045	10·31	10·46	10·895	S.W.	S. var.	S.E.
December	1·950	1·889	1·242	2·64	2·590	2·586	4·100	3·94	4·095	3·90	5·04	3·92	6·53	3·062	4·223	6·46	6·70	6·94	N.E.	N.E.	N. & N.W.
Inches.....	49·134	36·195	35·422	55·16	52·410	67·678	83·970	63·82	79·249	96·47	121·90	52·11	105·93	77·719	110·329	127·40	143·51	87·795	S.W.	S.W.	S.W.
	From April 38·063	From April					From May 57·640	From May				From April	From April 76·01				From June 84·98				

Table V.

Wet days in 1846.

1846.	Whitehaven.	The Flosh.	Cocker-mouth.	Keswick.	Loweswater.	Crummock Lake.	Wastdale Head.	Eskdale.	Grasmere.	Troutbeck.	Langdale Head.	Seathwaite.
January	22	25	26	21	20	21	28	...	26	23	28	25
February.....	15	14	16	13	13	15	18	...	15	16	16	15
March.....	18	19	23	23	18	21	25	...	21	20	21	23
April	17	21	24	22	16	22	23	19	21	20	22	21
May	12	13	15	13	14	12	14	15	12	13	13	14
June	9	10	12	10	11	11	11	9	9	9	9	11
July	24	24	28	27	22	23	27	27	21	22	23	25
August	13	13	18	17	18	18	22	19	14	17	15	16
September ...	12	13	13	11	11	14	11	11	10	9	11	12
October	23	23	23	24	24	25	23	23	23	20	23	24
November ...	18	17	19	18	17	17	17	17	16	18	16	17
December ...	17	16	17	14	14	17	15	15	14	7	16	16
Days	200	208	234	213	198	216	234	155	202	194	213	219

Table VI.

Temperature at Seathwaite, taken by Self-registering Thermometers made by Watkins and Hill.

1846.	Maximum.	Minimum.	Mean.	Mean at 9 A.M.	On Grass. Radiation. Maximum.	On Grass. Radiation. Mean.
	°	°	°	°	°	°
March.....	48	21	38·84	37·39	18	
April	60	30	43·68	42·78	12	4·61
May	65	41·5	52·47	52·54	18	8·71
June	84	44	64·83	63·87	23	11·80
July	79	50·5	59·41	58·22	12·5	
August	78	47	62·03	60·58	20	
September ...	76	40	58·20	56·16	21	12·00
October	61	30	47·91	47·50	17·5	
November ...	54	26	43·68	43·21		
December ...	47	14·5	32·13	32·05		
	84	14·5	50·318	49·43		
	At Whitehaven ...		52·295			
	Difference		1·977			

Table VII.

Synopsis of the Fall of Rain in the Lake Districts of Cumberland and Westmoreland in the year 1847.

No.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.*	IX.	X.	XI.	XII.	XIII.	XIV.	XV.	XVI.	XVII.	XVIII.	XIX.	XX.	
	Whitehaven.											Eskdale.			Westmoreland.			Borrowdale.			
																		Seathwaite.			
1847.	High Street, 90 feet above the sea.	Round Close, 480 feet above the sea.	*St. James's Church Steeple, 78 feet above the street.	The Pheels, three miles south of Whitehaven.	Cockermouth.	Bassenthwaite Halls, base of Skiddaw, 210 feet above the sea.	Keswick, 258 feet above the sea.	Vale of Gillerthwaite, Ennerdale, 280 feet above the sea.	Lowewater Lake, 336 feet above the sea.	Foot of Crummock Lake, 283 feet above the sea.	Gatesgarth, 326 feet above the sea.	Foot.	Head.	Wastdale Head, 166 feet above the sea.	The How, Troutbeck.	Grasmere, 180 feet above the sea.	Langdale Head, 250† feet above the sea.	In Garden, 6 inches above the ground.	In Field, 18 inches above the ground.	Stonethwaite Valley.	Prevailing winds at Seathwaite.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	
Jan.	1·873	1·528	1·365	2·09	1·49	2·31	3·082	3·50	3·135	3·75	4·34	3·66	3·84	3·99	4·235	4·957	4·54	6·29	6·16	5·67	S.W.
Feb.	1·827	1·406	1·370	2·01	1·62	1·73	2·074	4·91	3·400	3·20	5·07	2·89	3·96	6·40	4·789	6·066	8·07	8·27	8·03	5·40	N.W.
Mar.	1·370	1·191	1·098	1·23	·53	1·10	1·270	2·54	1·456	2·19	2·65	1·16	1·78	1·91	2·040	3·091	2·63	2·53	2·65	2·35	N.E.&S.W.
April	2·560	2·709	1·877	3·27	1·94	2·06	3·316	4·30	4·187	4·05	5·47	2·12	4·32	5·37	3·454	5·785	6·55	6·81	6·78	5·43	N.W. & W.
May	3·428	3·567	2·965	3·04	3·50	3·95	5·066	4·92	4·100	4·91	5·32	3·99	5·46	5·30	7·221	7·466	8·62	8·08	8·04	7·67	S.W.
June	2·912	3·297	2·406	3·54	3·12	3·59	3·622	4·83	4·225	5·48	5·86	4·23	5·95	6·62	4·518	5·178	6·71	7·27	7·18	5·21	N.W.
July	·776	·775	·824	1·20	1·17	1·13	1·410	1·98	1·110	1·65	1·99	1·67	3·21	3·80	1·779	2·513	2·71	3·32	3·17	2·35	S.W.
Aug.	4·496	4·494	2·814	4·98	4·76	3·76	4·070	6·12	6·572	7·57	9·14	5·33	5·82	7·79	4·779	4·015	8·49	10·48	10·31	7·78	W.
Sept.	3·738	3·478	2·367	4·21	4·57	4·40	5·324	9·34	6·594	6·99	10·13	6·68	8·14	11·94	4·436	8·278	11·34	13·28	12·80	10·00	N.W.
Oct.	5·407	5·102	3·840	6·56	5·47	5·13	8·806	10·50	8·835	11·96	16·83	8·30	10·27	13·53	12·922	14·022	17·75	20·52	20·09	17·57	S.W. var.
Nov.	7·937	8·235	5·393	8·70	8·10	8·70	11·420	17·10	14·225	17·71	22·86	10·59	13·25	17·54	15·904	17·558	18·19	21·85	21·26	20·83	S.W.
Dec.	6·597	6·241	4·394	6·97	6·28	6·59	8·826	10·09	8·457	12·86	16·59	8·04	8·93	12·15	11·927	14·409	17·35	20·54	20·33	15·95	S.W.& S.E.
1847	42·921	42·023	30·713	47·80	42·55	44·45	58·286	80·13	66·296	82·32	106·25	58·66	74·93	96·34	78·004	93·338	112·95	129·24	126·80	106·21	S.W.
1846	49·134	.....	35·422	55·16	52·41	.....	67·678	83·87†	79·249	96·47	121·90	.....	.....	106·93	77·719	110·329	127·40	143·51			
1845	49·207	.....	33·489	53·00	46·93	.....	62·202	76·88	69·542	87·48	124·13	.....	.....	108·55	76·305	121·008	136·00	151·87			
1844	36·723	.....	27·862	39·31	.....	.....	40·629	54·62	.....	.....	.....	.....	.....	.....	51·986						

* St. James's Church is forty-nine yards distant, in a direct line, from the gauge in my garden.

† In 1844, 1845 and 1846, the Ennerdale gauge was stationed at Bowness, about the middle of the lake; in 1847 the fall was measured at Gillerthwaite, about two miles *above the head* of the lake, and near the foot of Red Pike and the Pillar Mountain. During eight months of 1846 registers were kept at both stations, and the results show an excess of about one-ninth at the higher station.

** The Roman Numerals at the head of the Table refer to corresponding numbers in the accompanying map of the Lake Districts, so that the position of any one of the gauges may be seen at a glance.

Table VIII.

Wet Days.

1847.	Whitehaven.	The Flosh.	Cockermouth.	Keswick.	Lowes water.	Buttermere.	Wastdale.	Grasmere.	Langdale.	Troutbeck.	Bassenthwaite.	Seathwaite.	Stonethwaite.
January ...	13	15	15	14	13	14	14	15	15	10	11	13	14
February...	15	14	13	10	10	12	16	12	12	12	12	10	10
March.....	12	13	17	14	13	15	20	15	12	12	19	14	14
April	17	18	20	17	17	14	20	17	17	17	18	16	16
May	20	16	20	22	20	22	23	23	21	18	20	23	21
June	14	14	17	17	15	17	16	18	20	18	20	15	15
July.....	8	8	11	10	9	9	16	10	12	9	10	13	11
August ...	18	17	21	18	18	18	20	15	18	16	16	17	15
September.	20	17	22	24	21	21	24	22	23	21	21	23	23
October ...	14	15	16	18	17	17	17	19	17	18	14	19	17
November..	22	20	20	22	21	22	23	20	24	21	20	21	21
December..	18	16	18	18	16	18	24	18	18	16	18	18	18
1847	191	183	210	204	190	199	226	204	209	188	199	202	195
1846	200	208	234	213	198	216	234	202	213	194	.....	219	
1845	193	175	212	195	195	202	211	196	.....	180	.....	211	

Table IX.

Returns of the Mountain Gauges from March 1846 to November 1847 inclusive.

No.	XXI.	XXII.	XXIII.	XXIV.	XXV.	XIV.	XIII.	XXVI.	XVIII.	Prevailing winds.		
1846.	Sca Fell, 3166 feet above the sea-level.	Great Gable, 2925 feet above the sea.	Sparkling Tarn, 1900 feet above the sea.	Top of Stye Head Pass, 1290 feet above the sea.	Brant Rigg, 500 feet above the sea.	The Valley. Wastdale, 160 feet.	Valley* on south side of Sca Fell, Eskdale, height unknown.	Summit of Seatollar Common, Borrowdale, 1334 feet.	The Valley, Seathwaite, 242 feet above the sea.	At the sea coast, two daily observations.	At Crummock Lake, two observations daily.	At Seathwaite, Borrowdale, one observation daily.
	in.	in.	in.		in.	in.	in.	in.	in.			
March.....	6·51	7·47	14·94	} 21·62†	.....	10·35	.....	14·20	17·85	S.W.	S.W.	S.W.
April	4·39	4·83	5·38		.....	6·59	.....	7·53	7·70	N.E.	E.	S.E.
May	2·40	3·13	4·11		.....	3·65	.....	2·75	4·40	S.W. var.	W.	S. to W.
June	5·00	7·60	6·55	6·26	.....	5·33	.....	5·70	6·42	S.W. var.	W. & S.W.	S.W.
July	14·38	16·87	22·73	17·76	.....	16·82	.....	18·35	20·80	S.W.	S.W.	S.W.
August	7·05	8·65	12·03	11·03	.....	8·96	.....	8·15	10·58	N.E.	S.E.	S.E.
September ...	3·22	3·32	5·06	4·22	.....	3·79	.....	3·75	4·60	S.E.	S. to W.	W.
October	13·40	12·82	20·35	15·35	.....	15·75	.....	17·42	25·43	S.W.	S.W.	S.E. & N.W.
November to 1847 April	18·27	18·13	31·82	32·52	30·22	32·79	27·04	27·51	41·06	Variable. East and west, nearly equal.	Variable. East and west, nearly equal.	Variable to N.W.
May	6·16	5·56	7·59	7·56	5·96	5·30	5·46	7·13	8·08	S.W.	S.W.	S.W.
June	5·05	6·57	8·13	7·12	5·84	6·62	5·95	5·71	7·27	N.W. & N.E.	N.W. & W.	N.W. & S.W.
July	3·25	3·10	4·15	3·66	2·65	3·80	3·31	2·50	3·32	S.W. & N.W.	S.W. & N.W.	S.W.
August	8·39	7·90	12·00	10·22	7·48	7·79	5·82	10·38	10·48	W.	W.	W.
September ...	8·75	9·22	12·43	10·92	9·03	11·94	8·14	12·06	13·28	N.W.	N.W. & W.	N.W.
October	12·38	10·95	18·00	17·50	12·97	13·53	10·27	19·02	20·52	S.W.	S.E. to S.W.	S.W. var.
November ...	9·55	10·86	22·64	20·00	12·50	17·54	13·25	18·07	21·85	S.W.	S.	S.W.
In 21 months.	128·15 From Nov. 1846. 71·80	136·98	207·91	185·74	86·65 From Nov. 1846.	170·55 From Nov. 1846. 99·31	79·24 From Nov. 1846.	180·23	223·64	S.W.	S.W.	S.W.

* Sca Fell lies between these two valleys, but is considered to belong to the Eskdale Range. Wastdale faces the north, and Eskdale Head the south side of the mountain.

† The gauge above Stye Head Pass was not erected till the end of May, and in order to avoid confusion in comparing the total receipts, it is thought best to estimate the quantity up to the time the other instruments were fixed.

The following Table exhibits the *particulars* of rain obtained from the Mountain Gauges, between November 1846 and April 1847 (inclusive), during which period the water in the receivers was more or less frozen. The *total* quantities only are inserted in the preceding Table.

TABLE X.

1846.	Sca Fell.	Great Gable.	Sparkling Tarn.	Stye Head.	500 feet.	Valley. Wastdale.	Seatollar Common.	Valley. Seathwaite.
	in.	in.	in.	in.	in.	in.	in.	in.
Nov. 30.	Solid.	Solid.	9·40	7·32	13·90†	8·59	13·63	10·46
Dec. 31.	1·80	3·90	6·50	7·13		6·53		6·70
1847.								
Jan. 31.	Solid.	Solid.	Solid.	Solid.	2·00	3·99	Solid.	6·29
Feb. 15.	Solid.	Solid.	2·70	4·80	.....	6·40 to 28th.	Solid.	8·27 to 28th.
March 8.	Solid.	Solid.	0·58	5·47	6·56	.....	7·71	
March 22.*	13·33	11·08	6·68	0·86	.....	1·91 to 31st.	1·02 to 31st.	2·53
April 10.	Solid.	Solid.	3·41	4·10	5·05			
April 30.	3·14	3·15	2·55	2·84	2·71	5·37	5·15	6·81
In six months	18·27	18·13	31·82	32·52	30·22	32·79	27·51	41·06

TABLE XI.—Showing the proportion which obtains between the quantity of rain deposited on the Mountains and in the Valley in the Summer months.

1846.	Sca Fell, 3166 feet.	Great Gable, 2925 feet.	Sparkling Tarn, 1900 feet.	Stye Head, 1290 feet.	The Valley.	Borrowdale. Seatollar Common, 1334 feet.	Borrowdale. Valley.
					Wastdale.		Seathwaite.
	in.	in.	in.	in.	in.	in.	in.
May	2·40	3·13	4·11	3·80	3·65	2·75	4·40
June	5·00	7·60	6·55	6·26	5·33	5·70	6·42
July	14·38	16·87	22·73	17·76	16·82	18·35	20·80
August	7·05	8·65	12·03	11·03	8·96	8·15	10·58
September	3·22	3·32	5·06	4·22	3·79	3·75	4·60
October	13·40	12·82	20·35	15·35	15·75	17·42	25·43
1847.							
May	6·16	5·56	7·59	7·56	5·30	7·13	8·08
June	5·05	6·57	8·13	7·12	6·62	5·71	7·27
July	3·25	3·10	4·15	3·66	3·80	2·56	3·32
August	8·39	7·90	12·00	10·22	7·79	10·38	10·48
September	8·75	9·22	12·43	10·92	11·94	12·06	13·28
October	12·38	10·95	18·00	17·50	13·53	19·02	20·52
In twelve months..	89·43	95·69	133·13	115·40	103·28	112·92	135·18

TABLE XII.—Showing the proportion in the Winter months.

1846.	Sca Fell.	Great Gable.	Sparkling Tarn.	Stye Head.	The Valley.	Borrowdale. Seatollar Common.	Borrowdale. Valley.
	in.	in.	in.	in.	in.	in.	in.
March	6·51	7·47	14·94	.....	10·35	14·20	17·85
April	4·39	4·83	5·38	17·82	6·59	7·53	7·70
November to April	18·27	18·13	31·82	32·52	32·79	27·51	41·06
November	9·55	10·86	22·64	20·00	17·54	18·07	21·85
In nine months ...	38·72	41·29	74·78	70·34	67·27	67·31	88·46

* March 22, the gauges were free of ice; on the 31st they were again frozen up.

† Estimated in the same proportion as the other months bear to the valley, this gauge not being erected till December 31st, 1846.

Table XIII.

Temperature at Seathwaite, taken by Self-registering Thermometers made by Watkins and Hill.

1847.	Absolute		Mean of maximum.	Mean of minimum.	Mean at 4 feet.	Mean at 9h A.M.	Thermometer on grass.				Prevailing winds.	At Crummock Lake.	At the Coast.
	Maximum.	Minimum.					Minimum.	Mean.	Radiation.				
									Maximum.	Mean.			
January	46°·5	20°	38°·19	31°·41	34°·80	34°·71	.....	.....	.....	.....	S.W.	S.E.	S.E.
February	50	20	39·46	30·19	34·82	34·25	.....	.....	.....	.....	N.W.	Variable.	Variable.
March	57	21	45·71	35·60	40·65	39·82	.....	.....	.....	.....	N.E. to S.W.	S.E.	S.
April	56	25	47·44	35·83	41·63	41·90	18	30·12	14	5·63	N.W. & W.	S.W.& N.W. var.	S.W.& N.W.var.
May	72	37	57·67	46·87	52·27	51·90	30·4	40·43	15·5	6·44	S.W.	S.W.	S.W.
June	81	40	65·18	50·96	58·07	57·95	30·5	41·93	19·5	9·03	N.W.	N.W. & W.	N.W. & N.E.
July	80·5	51	72·16	57·45	64·80	64·98	38	47·17	20	10·27	S.W.	S.W. & N.W.	S.W. & N.W.
August	73	38·5	65·67	51·25	58·46	58·12	32	45·34	14	6·75	W.	W.	W.
September	63	31·3	56·78	45·16	50·97	50·91	26	40·50	9·5	4·79	N.W.	N.W. & W.	N.W.
October	64	34	53·72	43·51	48·61	47·88	27	38·80	10	4·71	S.W. var.	S.E. to S.W.	S.W.
November	56	20	49·55	41·06	45·30	45·26	17	37·93	8	3·20	S.W.	S.	S.W.
December	54	21	43·19	35·22	39·20	38·93	15	32·53	8	3·15	S.W. & S.E.	S. & S.E.	S.E. & S.
Means at Whitehaven.	62·7	29·9	52·89	42·04	47·46	47·21	.....	.....	.....	.....	S.W.	Westerly.	S.W. var.
	62·2	33·7	53·85	43·50	48·68								
Difference ...	0·5	3·8	0·96	1·46	1·22								

Note.—The Lake District gauges are 5 inches in diameter; they are all of the same form and construction, and are elevated about 18 inches above the surface. [During 1844, 1845 and part of 1846, they were raised only 6 inches above the ground: in the course of the latter year they were altered to their present height. From a series of daily observations made in 1847 (vide Table) at Seathwaite, it appears that at 18 inches a gauge receives about $2\frac{1}{4}$ per cent. less rain than at 6 inches above the surface.] The funnel rims are of stout sheet brass, so that the apertures cannot readily lose their circular form. The metres (Howard's) were all made by Mr. Bate of the Poultry, London. The rain (except at five stations) is read off daily at nine o'clock A.M., and each day is accounted wet in which any appreciable deposition is found in the instrument. The rain at St. James's Church Steeple, at Gatesgarth and Eskdale Head, is measured weekly; and at Round Close and Gillerthwaite once or twice a month.

The gauges at and in the vicinity of Whitehaven, are 8 inches in diameter, and the metres show distinctly each separate thousandth of an inch.

Before concluding to use a gauge of 5 inches diameter for the Lake Districts, I placed one of Howard's gauges in my garden, within a few yards of the 8-inch pluviometer, and measured the contents of each every morning for six months. From the 1st of August 1843 to the 31st of January 1844, the gauge of 8-inches aperture received 23·997 inches, and the 5-inch gauge 23·765 inches I attribute this trifling difference to the circumstance of the larger gauge-metre being graduated to $\frac{1}{1000}$th of an inch, whilst the smaller only indicates $\frac{1}{100}$th, or half a hundredth of an inch.

Remarks.

1845.—At Seathwaite, there have been thirty-one days in which the fall was between 1 and 2 inches; fifteen days between 2 and 3 inches; five days between 3 and 4 inches; one day between 4 and 5 inches, and one day between 6 and 7 inches.

On the 27th of November, at nine A.M., there was measured at Seathwaite 6·62 inches, and on the 26th and 27th nearly 10 inches, being unquestionably the greatest quantity of rain which has ever been recorded in the same period in the British Islands.

At Langdale Head, in Westmoreland, the fall on the 27th was 6·28 inches, and on the 26th and 27th nearly 9 inches.

On the 22nd of April 1792, Dr. Dalton measured 4·592 inches at Kendal, a remarkably wet locality; but I find on inquiry that the greatest fall at that place in twenty-four hours, during the present century, is rather short of 3 inches.

Of the total quantity of rain measured in the Vale of Borrowdale in 1845, 106·58 inches fell in the six months of January, March, August, October, November and December; and nearly 46 inches in the two latter months. The quantity in December, at some of the stations, is more than falls at many places in England during a whole year.

The fall at Seathwaite is more than three times the quantity at Whitehaven, one

of the wettest towns in the kingdom. It exceeds the fall at Leeds by six times; at Culloden by five and a half times; at Doncaster and Highfield House, Nottinghamshire, by five times; at Cirencester and Arbroath by five and a quarter times, and at Makerstoun near Kelso, by more than seven times.

Seathwaite exceeds Doncaster in January by fifteen times, in November by twenty-one times, and in December by nine and a half times. It exceeds the quantity at York in January by 16 inches, or twenty times; in March by nine times, and in November by twenty times. It exceeds Dublin in March by fourteen times, in April by thirteen times, in October by five times, and in November by seven times.

1846.—At Seathwaite there have been thirty-six days in which the quantity of rain was between 1 and 2 inches; six days between 2 and 3 inches; five days between 3 and 4 inches; one day between 4 and 5 inches, and one day between 5 and 6 inches. At Langdale Head there have been thirty-eight days of the first, five days of the second, and four days of the third class; and one day wherein the fall exceeded 4 inches. The rain at Seathwaite on the 3rd and 4th of March amounted to 6·86 inches; and on the 9th and 10th of October the fall was upwards of 9 inches. On three days of the latter month there fell 12·17 inches.

Of the total fall of rain at Seathwaite in 1846 (143·518 inches), 103·24 inches fell to the share of January, February, March, July, August and October; the other six months received much less than in the previous year. In November and December 1845, there fell 45¾ inches; in the corresponding months of 1846, the fall but slightly exceeds 17 inches. The table shows, that whilst the lake district stations generally have received *more* rain than in 1845, the deposit in the five wettest localities is somewhat *less* than in the previous year.

1847.—A glance at the first table will show that during the year 1847 much less than an average quantity of rain has fallen*; indeed, had it not been for the enormous downfall in October, November and December, the past year would have been one of the driest on record in this part of the country. At the close of September the fall in the lake districts was from one-third to one-fourth less than the average of the two preceding years: thus, Seathwaite was 29·59 inches, or nearly one-third; Wastdale 18·76 inches, or one-fourth; and Gatesgarth 34·96 inches, or *more* than one-third short of the average of 1845 and 1846, for the same period. But the year 1847 is memorable for the remarkable fact, that as much or nearly as much rain fell in the last three months as descended during the other nine months of the year. At the following stations, the quantity from October to December inclusive is considerably more than one-half of the whole annual depth.

	January to October.	October to December.
Buttermere	39·79	42·53
Gatesgarth	49·97	56·28
Troutbeck	37·25	40·75
Stonethwaite	51·86	54·35

* With some few exceptions the deficiency appears to have been general over the kingdom.

At Seathwaite there have been thirty-two days wherein the quantity of rain was between 1 and 2 inches, five days between 2 and 3 inches, five days between 3 and 4 inches, one day between 4 and 5 inches, and one day between 5 and 6 inches. At Langdale Head there have been thirty days of the first, three days of the second, and five days of the third class (3 to 4 inches).

There was hail in the lake districts on the 8th of June and on the 25th of July. The last traces of snow disappeared from the mountains on the 1st of June, and the first appearance of hoar frost was on the 27th of September.

I purposely postpone any remarks on the temperature of the lake districts till the next report, when I hope to make some extensive comparisons with other and widely different localities. The temperature of these valleys is much higher than is commonly imagined. The observations both in 1846 and 1847 were taken with great care, and I have no doubt of their correctness. The radiation from the earth is much greater in summer than at the coast; but in winter it appears to be so exceedingly small, that I have thought it best to omit the results for those months in the table till future observations have proved their accuracy or otherwise.

The Mountain Gauges.

The mountain gauges are on pretty much the same construction as those in the valleys, but the receivers are much more capacious, being calculated to hold nearly 80 inches of water. These gauges are, with one exception, stationed on the high mountains surrounding the vale of Wastdale.

Sca Fell, the highest mountain in England, stands on the south, and Great Gable on the north side of the valley. The gauge above Stye Head Tarn is on the shoulder of the Gable at the eastern extremity of the vale: Sparkling Tarn is about 600 feet above the top of Stye Head Pass, in a southerly direction, and 1260 feet higher, bearing south-west, are Sca Fell Pikes.

The bearings of the several stations from the gauge at Wastdale Head are as follow:—Sca Fell, S.; Gable, N.N.E.; Stye Head, N.E.; Sparkling Tarn, E. by N.; and Seatollar Common in Borrowdale, N.E., distant four and a half miles in a direct line.

The gauges on Sca Fell, Gable and Seatollar, are on the extreme summits of these mountains, and the whole of the instruments are freely exposed to the action of wind and rain from almost every point of the compass.

Appended to the tables for 1846 I find the following remark:—"It would be premature, from the scanty data before me, to draw any decided inference as to the gradation in the quantity of rain at these great elevations above the sea. But it seems probable that in mountainous districts the amount of rain increases from the valley upwards, to an altitude of about 2000 feet, where it reaches a maximum; and that above this elevation it rapidly decreases." The Table for 1846 exhibited the rain-fall of the summer months only, but the additional returns of 1847, obtained in every variety of season, confirm the above deductions in every essential particular.

so that we may fairly assume the combined results to be indicative of a physical law, so far at least as relates to the particular locality in question. Thus, in twenty-one months,

The Valley	160 feet above the sea,	has received	170·55	inches.
Stye Head	1290	„ „	185·74	„
Seatollar Common .	1334	„ „	180·23*	„
Sparkling Tarn . .	1900	„ „	207·91	„
Great Gable . . .	2925	„ „	136·98	„
Sca Fell	3166	„ „	128·15	„

An apparent exception to this law occurs in the gauge stationed at Brant Rigg, about midway between the top of Stye Head and the vale of Wastdale, at an estimated height of 500 feet above the sea, and which in last year has received about one-eighth, or twelve and three quarters per cent. less rain than the valley. This is the only one of the gauges situated on the *slope* of a mountain; it is on the windward side, and I imagine that in such a position, eddies or counter currents are produced in windy weather, which cause a less quantity of water to be deposited in the instrument than is due to the elevation. We know that all sloping roofs, from the same cause, materially diminish the receipts of rain-gauges.

It will be observed that the amount of water received by the Seatollar gauge is invariably less than the deposit in the adjacent vale of Seathwaite, and the deficiency is pretty equable in every month of the year. I am unable to give any satisfactory reason for this apparent anomaly, or to account for the very great excess of rain in this valley over all others in the lake districts. As the gauge on Seatollar is two or three miles distant in a direct line from the others, the near approach of its receipts to the Stye Head gauge, about the same elevation, is rather remarkable. In 1846 the Seatollar exceeded the Stye gauge in quantity, which it should do if the assumed height be correct.

By referring to the table for the summer months, we find that between the 1st of May and the 31st of October, the gauge at 1290 feet has obtained nearly twelve per cent. more rain than the valley; at 1334 feet, nine and a half per cent. more; at 1900 feet, twenty-nine per cent. more; at 2928 feet, seven and a half per cent. *less*; and at 3100 feet, thirteen and a half per cent. less than the valley.

In the winter months (November to April inclusive) the gauge at 1290 feet has received four and a half per cent. more than the valley, and at 1334 feet exactly the *same quantity* as the valley; at 1900 feet, eleven and a quarter per cent. more; at 2928 feet, thirty-eight and a half per cent. *less*; and at 3100 feet, forty-two and a half per cent. *less* than the valley. The difference in the proportion to the valley between the summer and winter half-year, as shown in the tables, is rather startling. When much snow falls, doubtless a considerable proportion is lost to the instrument, either

* The height of Seatollar Common has not been correctly ascertained.

by its being blown out of the funnel, or by the orifice getting choked up. But I do not think that this cause alone is at all adequate to account for the great comparative deficiency in the winter months, for there was very little snow on the mountain tops during the winter of 1846–47, less I am told by one of the oldest residents in the Fell Dales, than he almost ever remembers. At Whitehaven we had no snow worth naming, except on the night of the 23rd of December, when it covered the ground to the depth of nearly an inch, but disappeared in the course of the ensuing morning.

The late Mr. Crosthwaite of Keswick, by means of marks on the side of Skiddaw, and with the assistance of a telescope at his residence, made two or three daily observations on the height of clouds for several years, and it is clearly proved by his tables, that the clouds are lowest in the three first and three last months of the year*. Moreover, Dr. Dalton affirms in his "Meteorology," that the clouds are seldom a mile high (or little more than 1½ time the altitude of Sca Fell) in this climate in winter. Now the Doctor here probably alludes to, or at least includes, the most elevated clouds, such as the cirri, and some varieties of the cirrostratus. But there can be no doubt, that between the months of November and March, the under surface of the nimbus or rain-cloud (the lowest except the stratus) is *far* below the tops of our highest mountains, and I have reason to believe, not unfrequently, its upper surface also: when this is the case, the gauges on Sca Fell, Gable, &c. will receive no rain at all, when it is descending abundantly in the valleys beneath. I have a well-authenticated instance of such an occurrence, even in the middle of summer. On the 5th of July 1846 (the hottest day in the year) this county was visited by a dreadful storm of thunder, lightning, hail and rain, which continued from two to half-past four o'clock in the afternoon. Two gentlemen who happened to be on the top of Skiddaw during this storm, state that, whilst the rain was pouring down in torrents in the valley, not a drop fell on the summit of the mountain. In this elevated position the sky was clear, and the atmosphere calm and untroubled, when below them the elemental war was raging with the most terrific fury. The spectators describe the scene as awfully grand, beyond conception. The lowness of the rain-cloud at this season is, I apprehend, the principal cause of the small quantity of rain in proportion to the valley, during the winter as compared with the summer months.

I shall conclude this paper with a few general remarks.

In the year 1836 or 1837, Mr. Beck of Esthwaite Lodge, about two miles to the westward of Windermere Lake, began to register the amount of rain in that neighbourhood, and between 1837 and 1844, the annual quantity varied from 60 to 86 inches. In 1843, Mr. Jefferies obtained 90 inches at Grasmere. The results at Esthwaite and Grasmere were received with astonishment by meteorologists, not unaccompanied by some degree of suspicion as to their correctness. Indeed it was with the view of removing all doubt on the matter, that in the year 1844 I was

* Dalton's Meteorology, 1796.

induced to begin the present series of experiments on rain amongst the hills of Cumberland. Yet 90 inches would now be thought a small quantity for some parts of the Lake District, even in a year of drought!!

I am frequently asked by persons unacquainted with such matters, what beneficial end I expect such a series of experiments to lead to, and what information I have gained in return for my loss of time and trouble. To this question I may reply, they have shown us, that at least 60 inches more rain is deposited in England than we were previously aware of; that 150 inches sometimes descends in the Lake District in a year, more than falls in most parts of the tropics with which we are acquainted, and sufficient to drown standing, two of the tallest men in Great Britain, one on the top of the other. They have further informed us, that 6½ perpendicular inches of water is sometimes precipitated from the atmosphere in twenty-four hours, and 10 inches in forty-eight hours, a quantity which would be thought large for any two consecutive months in most parts of England. We have further ascertained that the almost incredible depth of 30 inches occasionally descends in a single month; a fall nearly equal to the calculated average for all other parts of England in a year*. The experiments have, in short, enabled us to collect a number of new and curious facts, bearing on the quantity and very unequal distribution of rain in this island. We have also ascertained, with a high degree of probability, the law of the gradation in the amount of rain, at various intermediate points, between the valleys and the tops of our highest mountains.

A little consideration will greatly lessen our surprise at the enormous quantities of water deposited in the hilly districts of Cumberland and Westmoreland, and at the consequent unequal distribution of rain in the climate of Great Britain. To those unacquainted with these localities, it may be briefly stated, that the lake district valleys radiate from a series of mountains of slate and primitive rock, having the Gable, 2928 feet in height, as a nucleus or central point, and in the immediate vicinity of which are Sca Fell and Pillar, of the respective elevations of 3166 and 2893 feet; and Great End, Bowfell and Glaramara, not much inferior in altitude. These mountains are distant only about thirteen or fifteen miles, in a direct line from the Irish Channel, and as no hills intervene, they are consequently fully exposed to our wet and prevailing winds, which are the south-west.

The warm south-westerly current arrives at the coast loaded with moisture obtained in its transit across the Atlantic: now our experiments justify us in concluding, that this current has its maximum density at about 2000 feet above the sea level; hence it will travel onward until it is obstructed by land of sufficient elevation to precipitate its vapour; and retaining a portion of the velocity of the lower parallel of latitude whence it was originally set in motion, it rapidly traverses the short space of level

* This astonishing quantity fell at Seathwaite in February 1848, in twenty-five days, on several of which the amount was very trifling. The gauge at 6 inches above the surface received 30·55 inches, and that at 18 inches 29·98, or 30 inches nearly.

country, and with little diminution of its weight or volume; but on reaching the mountains, it meets with a temperature many degrees lower than the point at which it can continue in a state of vapour; sudden condensation consequently ensues in the form of vast torrents of rain, which in some instances must descend almost in a continuous sheet, as when 9 or 10 inches are precipitated in forty-eight hours.

When we reflect that a warm moist current, perhaps only 3° or 4° above the point of saturation, in coming in contact with the mountain ridge, probably meets with a stratum of air 10° or 15° lower than its own inherent temperature, we shall cease to marvel that such quantities as four or five, or even six perpendicular inches of water should be deposited in these localities in the course of a few hours. The mountains are, in fact, huge natural condensers, destined to force from the atmosphere the mighty volumes of water requisite for the supply of our lakes and rivers.

I have before stated that I am unable to offer any satisfactory reason for the great excess of rain at Seathwaite over all the other valleys; judging from its situation, I should, *à priori*, have looked for the greatest fall at Wastdale Head, as it is surrounded by the highest mountains, and the valley opens out fairly to the south-west. But the maximum quantity is found to obtain, not where theory would indicate, but in the very identical spot where it is most required,—in the vale of Borrowdale, which affords the principal supply of water to the extensive and picturesque lakes of Derwent and Bassenthwaite; thus adding one more to the multitudinous instances which surround us, of the wonderful adaptation of external nature to the physical wants and requirements of man.

Whitehaven, March 15, 1848.

VIII. *An Investigation on the Chemical Nature of Wax.*

By Benjamin Collins Brodie, *Esq.*
Communicated by Sir Benjamin C. Brodie, *Bart., F.R.S. &c.*

Received May 11,—Read November 23, 1848.

III. *On Myricin.*

I HAVE placed the investigation of the Chinese wax between that of the cerotic acid and of the residue of the bees'-wax which remains after that substance has been separated from it. By the saponification of this Chinese wax we procure, as I have shown, an acid identical with the cerotic acid from bees'-wax, and also the alcohol of this acid, so that the chemical history of these substances is closely connected. We have moreover in the Chinese wax to deal with a substance found in nature in a state of great purity, the products of the decomposition of which by alkalies and by heat can readily be prepared and examined. The knowledge of the relation of these products to one another throws great light upon the nature of myricin, which is not a pure substance, and the chemical relations of which are complex.

I have stated that the first extracts of wax with alcohol give with acetate of lead an abundant precipitate in a hot alcoholic solution. This affords us a ready test of the presence of the cerotic acid. The wax may be long boiled with alcohol before the whole of the cerotic acid is removed. If however this process of boiling and decantation be continued, a time will come when the acetate of lead will cease to give any precipitate whatever in the hot alcoholic extract. The residue after this extraction I speak of as myricin. It is advisable to continue for two or three times the operation of boiling and decanting, even after the acetate gives no precipitate, the cerotate of lead not being entirely insoluble in the hot solution.

The myricin thus prepared is a greenish substance of about the consistency of wax, uncrystalline, still possessing a slight smell of wax, and of a melting-point of 64° C. This substance is hardly acted on by dilute potash. It is however saponified by boiling with strong potash, and more readily by an alcoholic solution of the alkali. The saponification may also be effected by melting it with hydrate of potash, as in the case of the Chinese wax. The products are the same in whichever way the operation be conducted.

If the soap from the saponification of the myricin be treated in the same manner as the similar soap from the Chinese wax*, it also will be found to contain two substances, an acid and another substance which is contained in the ether with which the baryta salt is extracted. On attempting to purify these substances respectively

* Philosophical Transactions, 1848, Part I. p. 161.

by crystallization out of alcohol, ether or absolute alcohol, great variations in the melting-point both of the acid and of the basic substance will be observed. And careful observation shows that these are not, as in the case of the Chinese wax, substances in a state of comparative chemical purity, but are mixtures, both in the case of the acid and of the other matter, of at least two bodies difficultly separable from one another. It is the separation of these substances which gives a peculiar difficulty to the investigation of the nature of myricin.

Although the acid and basic products of the saponification may thus, as in the case of the Chinese wax, be separated by precipitation of the soap by a baryta salt, in the case of the bees'-wax these substances admit of a simpler method of separation, without which method, so difficult is it to wash perfectly out the baryta salt, that I question whether the substances could be obtained pure. The soap, in whatever way the saponification may have been effected, and after the alcohol, if any, used for the saponification has been distilled off, is to be dissolved in a large quantity of water, and the boiling solution decomposed by an acid. The melted mass which results from this operation, after having been repeatedly boiled out with water, is to be dissolved in a large quantity of hot alcohol. An abundant precipitate appears in the cold fluid from which the solution is to be filtered, and the precipitate repeatedly redissolved and recrystallized out of alcohol. The precipitate will at length be found to consist, almost entirely, of the basic portion of this waxy matter. The alcoholic solution contains the acid.

I shall proceed to give the simplest method by which the pure substances may be obtained, and those experiments which I have made upon their constitution, which I think can leave no doubt upon the mind of the chemist as to the true nature of that matter of which by far the greater portion of the myricin and, indeed, of the wax itself consists.

The first separation of the products of saponification may be made as I have stated, by combining the acid with baryta and washing out the resulting salts with ether; the basic portion of the products may be obtained as pure by this as by the other method.

Melissin.

If the substance contained in the etherial solution, with which the baryta salt is washed out, be crystallized out of ether or alcohol, the melting-point will be considerably raised, from below 70° C. to above 80°, by repeated crystallization. The difficulty with which the melting-point was raised, made it evident that the substances contained in the solution were to be separated only by long crystallization and a careful attention to the variations of the melting-points. I made various experiments to discover a satisfactory method of purification. At length I found that if the etherial solution be filtered while yet warm, and when only a small portion of matter has crystallized out, a substance remains on the filter of a melting-point of 85° C. of a satiny lustre, and of highly crystalline appearance. It is with difficulty that

even a small portion of substance can be thus obtained, and it is necessary to use, during the filtration, a hot water apparatus to prevent the precipitation of the whole matter dissolved. I have never been able to succeed in further raising the melting-point of this body, and therefore regard it as pure. In this condition it crystallizes on cooling from the melted state, and its crystallization is marked by striæ parallel to the line of cooling; it being in all respects, but the melting-point, similar in appearance to cerotin as procured from Chinese wax.

I give this method of preparing this substance as it was the first I adopted, and as it can thus be procured in a high state of purity. I afterwards however discovered the use of rectified coal naphtha as a solvent for these substances, and by far the best and simplest method of procuring the body is by crystallization out of that solvent, of the precipitate from the alcoholic solution which I have before mentioned, as procured by dissolving in alcohol the wax matter obtained by decomposing by an acid the soap from the myricin. By alcohol the basic portion of the saponified myricin is separated from the acids. By naphtha the substance of 85° melting-point is separated from another and probably an analogous body, of which I shall speak hereafter.

This substance gave to analysis the following numbers. The result is the same in whatever way the substance is prepared.

	Substance.	CO_2.	HO.
I.	0·2685 grm. gave	0·8075	0·341
II.	0·2597 grm. gave	0·7839	0·3326
III.	0·278 grm. gave	0·84375	0·35325
IV.	0·2584 grm. gave	0·7812	0·325
V.	0·2511* grm. gave	0·7595	0·3215
VI.	0·2617† grm. gave	0·7870	0·3295

which give in 100 parts—

	I.	II.	III.	IV.	V.	VI.
Carbon	82·02	82·40	82·77	82·43	82·48	82·01
Hydrogen	14·11	14·25	14·11	13·97	14·22	13·99
Oxygen	3·87	3·35	3·12	3·60	3·30	4·00
	100·00	100·00	100·00	100·00	100·00	100·00

These analyses agree with the formula—

	Atomic weight.	Calculated.
C_{60}	360	82·19
H_{62}	62	14·15
O_2	16	3·66
	438	100·00

This substance I propose to call Melissin.

* This substance was procured directly from wax, from which it may be obtained and purified in the same manner as from the purified myricin; which is the simplest way of procuring the substance if the other products of saponification are not required.

† This substance was procured from the Ceylon wax mentioned in a former paper.

Melissic Acid.

Melissin, heated with lime and potash, as the similar experiment was made with cerotin*, is, like that body, converted into an acid. This acid has a similar appearance to the wax acid already described. It has however a much higher melting-point, namely, 88°–89° C. The preparation of the substance need not be again described.

	CO_2.	HO.
I. 0·2655 grm. gave	0·7764	0·3104
II. 0·2507 grm. gave (another preparation)	0·728	0·2507
III. 0·2508 grm. gave	0·7333	0·3077
IV. 0·2396 grm. gave (another preparation)	0·7026	0·2885
V. 0·258 grm. gave		0·3085

which give in 100 parts—

	I.	II.	III.	IV.	V.
Carbon	79·74	79·19	79·74	79·97	
Hydrogen	13·00	13·32	13·63	13·40	13·28
Oxygen	7·26	7·49	6·63	6·63	
	100·00	100·00	100·00	100·00	

These analyses agree with the formula—

	Atomic weight.	Calculated.
C_{60}	360	79·64
H_{60}	60	13·27
O_4	32	7·09
	452	100·00

I prepared the silver salt of this acid in precisely the same manner as was prepared the silver salt of the cerotic acid.

I. 0·6085 grm. gave	0·1175 silver.
II. 0·678 grm. gave	0·1315 silver.
III. 0·58625 grm. gave (another preparation)	0·11575 silver.

which give in 100 parts—

	I.	II.	III.
Silver	19·30	19·39	19·74

	CO_2.	HO.
I. 0·4619 grm. of the salt gave	1·0863	0·4464
II. 0·484 grm. of the salt gave	1·13375	0·471

giving in 100 parts—

	I.	II.
Carbon	64·13	63·90
Hydrogen	10·73	10·81
Oxygen and silver	25·14	25·29
	100·00	100·00

* Philosophical Transactions, 1848, Part I. p. 161.

These analyses lead to the formula $C_{60} H_{59} O_4 Ag$.

	Calculated.
C_{60}	64·38
H_{59}	10·55
O_4	5·77
Ag	19·30
	100·00

The formula therefore of the hydrated acid is $C_{60} H_{60} O_4$. This acid I call Melissic Acid.

Chlor-Melal.

By the action of chlorine on melissin a perfectly analogous result is obtained to that obtained by the action of chlorine on cerotin. The substance undergoes also a similar change in appearance, being converted into a resin.

The substance was prepared and analysed with a view to confirming the formula of the body.

	CO_2.	HO.
I. 0·4136 grm. gave	0·589	0·175
II. 0·4263 grm. gave	0·602	0·1835

which give in 100 parts—

	I.	II.
Carbon	38·83	38·51
Hydrogen	4·70	4·78
Oxygen and chlorine	56·47	56·71
	100·00	100·00

I. 0·6663 grm. gave 1·4821 grm. of chloride of silver equivalent to 0·3665 grm. of chlorine.

II. 0·6075 grm. gave 1·341 grm. of chloride of silver equivalent to 0·3316 grm. of chlorine.

III.. 0·6475 grm. gave 1·4375 grm. of chloride of silver equivalent to 0·3555 grm. of chlorine.

These determinations correspond in 100 parts to—

	I.	II.	III.
Chlorine	55·01	54·58	54·91

These analyses lead to the formula

$$C_{60} \left\{ \begin{matrix} H_{45·5} \\ Cl_{14·5} \end{matrix} \right. O_2.$$

C_{60}	38·50
$H_{45·5}$	4·86
$Cl_{14·5}$	54·90
O_2	1·74
	100·00

As in the case of the cerotin, by the action of chlorine two equivalents of hydrogen are removed without replacement by chlorine, while the further action is an action of substitution, the substance being the analogue of chloral.

The products of the distillation of melissin are analogous to those of the distillation of cerotin. The substance partly distils over unaltered, and is partly, with the loss of water, converted into solid hydrocarbon. Sulphuric acid also combines with it under the same conditions as with the other wax-alcohol.

Palmitic Acid from the Saponification of Myricin.

Melissin is soluble with such great difficulty, in every solvent suitable for washing out the baryta salt from the wax soap, that its separation from the acid cannot in this manner be effected. It may however be separated by simple crystallization. The alcoholic solution (p. 278) from which the melissin has crystallized out, after having been considerably concentrated and again filtered from any precipitate produced on cooling, contains hardly a trace, if any, of that substance. The acids are very soluble in alcohol, and it is only on great concentration that they crystallize from that solvent. The alcohol is to be distilled off to the point of crystallization, and the first portions only of the fat acid selected for the preparation of the pure substance. The acid is to be boiled with potash, combined with baryta, and washed out with ether.

On decomposing the baryta salt with hydrochloric acid, a fat acid separates, having the appearance of margaric or palmitic acid, which latter body is in truth the principal acid of the wax. It is however mixed with another acid of a lower melting-point, for which reason it is desirable, as I have mentioned, to use in its preparation only the first crystallization of the acid. From this other body it is separable with the greatest difficulty; but by long-continued crystallization from ether, an acid may be obtained of the melting-point of 62° C., beyond which point it cannot be raised. This acid gave to analysis the following results:—

	CO_2.	HO.
I. 0·2486 grm. gave	0·6877	0·278
II. 0·2605 grm. gave	0·7145	0·290
III. 0·2542 grm. gave	0·6937	0·2847

giving per cent.—

	I.	II.	III.
Carbon	75·42	74·80	74·43
Hydrogen	12·43	12·36	12·43
Oxygen	12·15	12·84	12·14
	100·00	100·00	100·00

The silver salt was made as in the other cases by precipitation from the ammoniacal solution of the acid.

I.	0·6885 grm. of this salt gave	0·2005	silver.
II.	0·66025 grm. of the same gave	0·1920	silver.
III.	0·623 grm. of another preparation gave	0·182	silver.
IV.	0·609 grm. of the same gave	0·17625	silver.
V.	0·671 grm. of another preparation gave	0·197	silver.
VI.	0·744 grm. of the same gave	0·2185	silver.

giving in 100 parts—

I.	II.	III.	IV.	V.	VI.
29·12	29·23	29·21	28·94	29·35	29·36

	CO_2.	HO.
I. 0·4458 grm. of the first preparation gave . . .	0·869	0·3495
II. 0·4463 grm. of the same preparation gave . .	0·870	0·3555
III. 0·5896 grm. of the second preparation gave . .	0·7545	0·3065

which correspond in 100 parts to—

	I.	II.	III.
Carbon	53·16	53·22	52·82
Hydrogen	8·70	8·85	8·75
Silver and oxygen	38·14	37·93	38·43
	100·00	100·00	100·00

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The silver salt is by no means insoluble in the ammoniacal solution, so that in the making of the salt by this method a certain separation of the substance is effected. If any impurity were presented, it probably would be detected on analysing the acid as again separated from the silver salt.

	CO_2.	HO.
I. 0·2523 grm. of the acid thus separated gave . .	0·6970	0·285
II. 0·228 grm. of the same gave	0·6255	0·257

giving in 100 parts

	I.	II.
Carbon	75·38	74·82
Hydrogen	12·56	12·52
Oxygen	12·06	12·66
	100·00	100·00

These analyses, as well as those of the acid previous to combination with silver, agree with the formula of palmitic acid, $C_{32}\,H_{32}\,O_4$, with which substance the melting-point of the acid also identifies it. The calculated numbers in parts per cent. of the acid and silver salt are—

C_{32}	192 . . .	75·0
H_{32}	32 . . .	12·5
O_4	32 . . .	12·5
	256	100·0
C_{32}	192 . . .	52·8
H_{31}	31 . . .	8·5
O_4	32 . . .	9·0
Ag	108·1 . . .	29·7
	363·1	100·0

Distillation of Myricin.

The discovery of the cerotic acid rendered it evident that in order to obtain the products of distillation of myricin, and especially the acids in a state of purity, it was necessary first to remove that body and to distil only the residue of the wax. I give the results of this experiment made with myricin. The first portions of the distillate consist almost entirely of acids, the latter of hydrocarbons. During the distillation a smell of butyric acid may be perceived. This however appeared to me to diminish when the boiling of the wax with alcohol had been very long continued. It is possible to effect nearly a complete separation of the acids and the hydrocarbons by distillation. It is however not advisable to proceed in this manner, but it is best after boiling the distillate with water to saponify the whole by potash. The soap may be removed by a syphon from the hydrocarbons which float on the surface.

Palmitic Acid from the Distillation of Myricin.

The acid, having been purified in the usual manner by washing out the baryta salt with ether, and the subsequent methods of purification, presents an appearance similar to the acids obtained by saponification. By crystallization the melting-point may be raised to 62° C.

	CO_2.	HO.
I. 0·2592 grm. of this acid gave	0·7165	0·2931
II. 0·250 grm. of this acid gave	0·6865	0·27925
III. 0·2775 grm. of this acid gave	0·75925	0·311

These analyses correspond in 100 parts to—

	I.	II.	III.
Carbon . .	75·39	74·89	74·61
Hydrogen .	12·58	12·40	12·45
Oxygen . .	12·03	12·71	12·94
	100·00	100·00	100·00

The silver salt of this acid, prepared as before, gave the following results:—

I. 0·5006 grm. of the substance gave . . . . . 0·1479 silver.
II. 0·2295 grm. of another preparation gave . . . . 0·0685 silver.

which correspond in 100 parts to—

	I.	II.
Silver	29·54	29·84

	CO_2.	HO.
0·3505 grm. of the same salt gave	0·6873	0·2758

which gives in 100 parts,—

Carbon.	53·47
Hydrogen.	8·74
Oxygen and silver	37·79
	100·00

These numbers prove the identity of the acid from the distillation of myricin with that obtained from the saponification of that substance (p. 283).

There are great difficulties in the way of obtaining even a sufficient quantity of this acid for the determination of its formula. To obtain even a very small portion of it of which the purity may be relied on, it is necessary to operate on a large quantity of the impure acid: for the preparation of this pure myricin is required, free from cerotic acid, which it is not easy to get in any quantity.

These difficulties have prevented me making any further experiments with this acid, the identity of which however with palmitic acid, as obtained by Fremy and Stenhouse from palm oil, and by Sthamer from Japan wax, is made out. I subjoin, for the sake of comparison, the silver determination of the silver salt of the palmitic acid as obtained by these chemists.

	Fremy*.			Stenhouse†.	Sthamer‡.
Silver, per cent. . .	29·60	29·23	29·42	29·28	29·51

This acid appears also to be the same as the acid obtained by Varrentrapp§ from the oxidation of oleic acid by means of lime and potash, which also had the melting-point of 62° C. The silver determinations of this acid gave as the per-centage of silver,

29·27 29·45 29·13,

numbers identical with my own.

Melen.

It is well known that one of the principal products of the dry distillation of wax is a solid hydrocarbon. Ettling, who first analysed this substance‖, concluded from its melting-point, analysis, and general appearance, that it was identical with paraffin, a hydrocarbon then recently discovered by Reichenbach in the products of the dry distillation of wood. The wax hydrocarbon has therefore borne the name of paraffin.

This substance was supposed, from the analyses of Ettling and J. Gay-Lussac, to be isomeric with olefiant gas.

Recently, however, this has been contested by Lewy, who analysed paraffin from various sources, and showed it, as he conceived, to contain a larger amount of hydrogen than had been previously supposed. In truth the average of his analyses gave,—

Carbon	85·03
Hydrogen	14·87
	99·90

numbers inconsistent with the old idea. The question however is, whether M. Lewy experimented with a pure chemical substance, for which there is no guarantee.

* Liebig's Annalen, vol. xxxvi. p. 45. Silver determinations, V. VI. VII.

† Ibid. p. 52. The mean of five determinations closely agreeing. This acid melted at 60° C.

‡ Ibid. vol. xliii. p. 342. The mean of three determinations.

§ Ibid. vol. xxxv. p. 209.

‖ Ibid. vol. ii. p. 259.

My own experiments confirm the analyses of Ettling, and the constitution originally assigned to the substance, to which theoretical considerations also lead. But I cannot see any reason to believe the wax hydrocarbon to be identical with the paraffin of Reichenbach. This name of paraffin has been applied indiscriminately to the whole class of solid hydrocarbons, which have, or have nearly the formula $C_n H_m$, the identity of which has been taken for granted, in the absence of any true knowledge as to the chemical nature of the substances from the decomposition of which by heat they are produced. The different melting-points however of these substances point out to us at once a distinction between them. The paraffin of M. Lewy melted at 46°·8. A specimen of the paraffin of wood given to me by Professor Liebig, and which that gentleman received from Reichenbach, its discoverer, melted at 43°·5 C.; Ettling's paraffin at 57° to 58° C. I confess it is difficult for me to conceive what substance in a state approaching to purity Lewy analysed from the wax having the melting-point he has given, since nothing is easier than to raise the melting-point of the paraffin from the wax to 56° C., although beyond this any change is effected with difficulty.

Cerin alone gives on distillation hardly a trace of this hydrocarbon, while it forms a principal product of the distillation of myricin. The palmitic acid is separated by saponification, and the general preparation of the substance is the same as in the similar case of the ceroten from Chinese wax, to which substance it is closely analogous. If the hydrocarbon from the distillation of the pure myricin, the acids having been boiled out with potash, be pressed out in a press between blotting-paper, it will have a melting-point of about 56° C. This can be raised by further crystallization out of ether to 60° C. The analysis of the substance in this condition shows the presence of some body containing oxygen, in addition to the hydrocarbon.

	CO_2.	HO.
0·2606 grm. of this substance gave	0·8094	0·3402

giving in 100 parts,—

Carbon	84·74
Hydrogen	14·51
Oxygen	0·75
	100·00

Another analysis gave similar results. This led me to prepare the substance in rather a different manner. The paraffin having been carefully pressed out in the manner described, was rectified over potassium, which destroys the oxygen compound. The distillate is perfectly white: it contains a little oil, which may again be pressed out. By crystallization out of pure ether, the melting-point may now be raised to 62° C. This substance was analysed.

	CO_2.	HO.
0·261 grm. gave . . .	0·8165	0·3393

giving in 100 parts,—

Carbon	85·31
Hydrogen . . .	14·44
	99·75

The formula $C_m H_m$ demands—

C_m	85·71
H_m	14·28
	99·99

The difference between the hydrogen calculated and found is only 0·16 per cent., which is as near to theory as such analyses can be expected to come. Cerotin melts at 81° C. The hydrocarbon I have called ceroten melts at 57° to 58°. Melissin melts at 85°. The wax hydrocarbon at 62° C., showing a precisely analogous difference in their melting-points. Owing to the numerous operations which are necessary before this hydrocarbon can be procured in a pure state, I have been unable to make further experiments with the pure substance. The analyses, however, the analogy of this other substance and the mode of its formation, can leave no doubt but that it is the hydrocarbon of the wax-alcohol $C_{60} H_{60}$, to which may be given the name of melen.

The Nature of Myricin.

The analogy of the products of the decomposition of myricin by alkalies and by heat, to those of the Chinese wax and of spermaceti under similar circumstances, would lead us to suspect that a similar relation exists between the substances to which these products are due. If, however, we take the numbers which have been obtained by analysis for this body, those for example of Ettling*, or those of Lewy†, and attempt from these to reckon out a formula which shall give a rational account of these decompositions, we find a considerable deficiency of carbon. I give one of Lewy's analyses, with which other analyses of himself and other chemists are sufficiently accordant‡.

Carbon	80·28
Hydrogen	13·34
Oxygen	6·38
	100·00

The formula $C_{92} H_{92} O_4$, which would account in a simple manner for the decompositions,—

$C_{32} H_{31} O_3$	$C_{32} H_{32} O_4$
$C_{60} H_{61} O$	$C_{60} H_{60}$
$C_{92} H_{92} O_4$	$C_{92} H_{92} O_4$

* Liebig's Annalen, ii. 267. † Annales de Chimie, xiii. 443. ‡ Ibid.

requires—

		Atomic weight.
C_{92}	81·65	552
H_{92}	13·60	92
O_4	14·75	32
	100·00	676

leaving a difference of one and a half per cent. of carbon, a difference too great to be attributed to any accidental error.

I have stated that the decompositions of the myricin are far from being so simple as those of the Chinese wax, and that in order to obtain either the acid or the wax alcohol, long and repeated crystallizations are necessary. This at once led me to the suspicion that the so-called myricin was no pure chemical substance, but a mixture of two or more bodies. Subsequent experiment confirmed this view.

The residue of the wax, after the cerotic acid has been boiled out by alcohol, melts at 64° C. It is but very slightly soluble in alcohol. Pure ether, however, will dissolve it without much difficulty. It crystallizes out of this reagent in light feathery crystals. The precipitate and the residue from the solution, evaporated to dryness, have different melting-points. I succeeded in this manner in raising the melting-point of the precipitate to 71°·5. This end may be more readily obtained by adding a small quantity of naphtha to the ether.

The following analyses were made of a substance of 72°, which after repeated crystallizations was precipitated on the filter out of the hot solution, the filter being kept hot by means of a hot water apparatus. I have not succeeded in raising the melting-point beyond 72°. The substance is now highly crystalline in appearance, which the impure myricin is not, and of about the consistency of wax. I regard it in this state as pure.

	CO_2.	HO.
I*. 0·2592 grm. of substance gave	0·7735	0·3135
II. 0·2243 grm. of substance gave	0·672	0·269

which give in 100 parts,—

	I.	II.
Carbon . . .	81·38	81·70
Hydrogen . . .	13·44	13·33
Oxygen . . .	5·18	4·97
	100·00	100·00

These numbers are very different from any which have been before obtained for any substance from the myricin, and different from those which I myself have ob-

* The thorough combustion of these waxes is difficult, and I have made many experiments to ascertain the best method of analysis. Bichromate of lead was the material generally employed. But when the combustion is made very slowly, I believe it to be complete even with oxide of copper alone. The greater number of such analyses in this investigation were made by my chemical assistant, Mr. L. Hoffmann, to whose care and skill I am much indebted.

tained for substances of a lower melting-point. The crystalline appearance marks the purity of the substance, and notwithstanding the slight difference in the hydrogen, I cannot but regard it as the body $C_{92} H_{92} O_4$, with the calculated formula of which, as given above, it sufficiently agrees. I must add that the substance is separable with extreme difficulty. The next precipitate from the solution from which the above substance had been separated, had a melting-point half a degree lower, and gave to analysis rather less carbon, namely, C. 81·0 per cent.

The greater part of the difficultly saponifiable portion of the wax appears to consist of the substance the analysis of which I have just given, and to which we may confine the name myricin. We have, however, clearly some other body present accompanying it, the products of the decomposition of which by potash are to be found with both the acid and the wax-alcohol procured by saponification of the impure substance, which, as I have said, render extremely difficult the preparation of these bodies in a pure state. I shall proceed to give some experiments which throw some true light upon the nature of this substance, although I cannot say that its history is satisfactorily made out. The solution of ether or naphtha (p. 278) from which the melissin of 85° has been separated, still contains a large quantity of substance of a similar appearance, but of a melting-point much lower than that of the melissin itself. Notwithstanding however the differences in the melting-point, analysis shows us but little or rather no difference in the constitution of the different portions of this substance. In the case for example of a substance melting at 78°·5 C.,—

	CO_2.	HO.
0·2522 grm. gave	0·764	0·324

which gives in 100 parts,—

Carbon	82·59
Hydrogen	14·27
Oxygen.	3·14
	100·00

In the case again of a substance melting at 72°,—

	CO_2.	HO.
0·249 grm. gave	0·75075	0·317

which gives in 100 parts,—

Carbon	82·22
Hydrogen	14·14
Oxygen	3·64
	100·00

Other analyses gave similar results.

These analyses do not differ seriously from one another, and give precisely the numbers of the melissin itself (p. 279). The numbers however are consistent with various formulæ besides that of the melissin. At 72° the melting-point is extremely constant. A portion of substance was obtained at this melting-point by repeatedly filtering the etherial solution from the melissin which first crystallized out of the hot liquid. A time arrived when there was no difference between the melting-point of the portion which first crystallized out of the hot solution and which was on the filter, and that which afterwards crystallized out of the fluid which had passed through. The melting-point in both cases was 72°. By heating with lime and potash, as in the case of melissin, this substance of 72° also affords an acid, which after the usual preparation, gives very different numbers to those of the melissic acid. This acid melts at 77°·5.

	CO_2.	HO.
I. 0·256 grm. gave	0·735	0·3015
II. 0·267 grm. gave	0·765	0·311
III. 0·2551 grm. gave	0·730	0·2995

giving in 100 parts,—

	I.	II.	III.
Carbon	78·28	78·14	78·05
Hydrogen . . .	13·09	12·94	13·05
Oxygen	8·63	8·92	8·90
	100·00	100·00	100·00

Between the second and third analyses the substance was twice crystallized out of ether. The substance dissolved by the ether had the same melting-point of 78° as the substance on the filter.

The silver salt of this acid gave the following numbers :—

	CO_2.	HO.
I. 0·5054 grm. of substance gave . . .	1·127	0·4572
II. 0·5182 grm. of substance gave . . .	1·1505	0·467

giving in 100 parts,—

	I.	II.
Carbon	60·80	60·56
Hydrogen	10·05	10·01
Oxygen and silver .	29·15	29·43
	100·00	100·00

I. 0·617 grm. gave on ignition . . . . 0·1375 silver.
II. 0·7315 grm. gave on ignition . . . . 0·1625 silver.

giving per cent.—

	I.	II.
Silver	22·28	22·21

These analyses perfectly agree with the formulæ for the acid, $C_{49} H_{49} O_4$.

	Calculated.
C_{49}	78·4
H_{49}	13·0
O_4	8·6
	100·0

	Calculated.
C_{49}	60·9
H_{48}	9·9
O_4	6·8
Ag	22·4
	100·0

If we compare the numbers of this acid with those of the substance from the oxidation of which it was derived, we shall see that it is impossible to account for the changes in the same simple manner as in other cases of such transformation. It would not be difficult to reckon out a formula that without great violence should account for it, but it is hardly worth while to do so, since notwithstanding the perfect agreement of the calculated and theoretical numbers, it is impossible to assert with certainty that either it or the body from which it is derived are pure chemical substances. There is too great a difficulty in the perfect separation of the melissin to lead us to hope that it can absolutely be removed by the method I have given. I failed in attempting to procure in larger quantities this substance of 72°. The melting-point was very constant at 75°, but on oxidizing a considerable quantity of this substance with lime and potash, acids were procured, which by crystallization were separable in the same manner as the substance from which they were derived, and the purification and perfect separation of which presented the same difficulties. I obtained in this way an acid having nearly the melting-point of 85°, the melting-point of melissic acid, and also an acid with a lower melting-point than 77°, but of which the melting-point was not so absolutely constant as to induce me to investigate it further. I give however these analyses, since they unquestionably prove the existence of some other body in addition to the melissin, in the products of the saponification of wax, which by oxidation is capable of passing into an acid belonging to the series $C_m H_m O_4$. Since it is only a pure body or a mixture of acids of this series which could give rise to the results I have given, and from the great difficulty of separation, the acid in all probability contains a very large number of equivalents of carbon, whether it have precisely the formula I have above given or not.

Mixed with the palmitic acid of 62°, is found another acid of a much lower melting-point, and which presents similar difficulties of separation from the palmitic acid to those of the substance mixed with the melissin from the melissin itself. This acid is very soluble in alcohol, unctuous to the touch, and of a very low melting-point. I do not, however, mean to assert that the other wax-alcohol exists in the wax in

combination with this unctuous acid, the presence of which is very probably due to another source.

This alcohol may possibly, as well as the melissin, be combined with palmitic acid, or it may be in some altogether different form in the wax. Even after long boiling with alcohol, the myricin has a slight wax smell, and it is possible that this unctuous acid is the product of the action of potash upon the oil which is one of the constituents of the wax, and from which I have in fact procured an acid of this nature. This oil, or rather grease, which was analysed by Lewy, is a very curious substance. The other constituents of the wax are, in a pure state, inodorous and crystalline, and to it the wax owes its tenacity and peculiar smell. I have made some experiments as to its nature, and procured from it also an acid and an unsaponifiable substance; I will not, however, here enter upon the matter, hoping at some future time to resume its investigation.

I must not omit to mention, with reference to the bees'-wax from Ceylon, of which I spoke in a former paper, and which contained no cerotic acid, that it possesses all the general characters of the other portion of the wax. Like the impure myricin, it contains more than one substance. The wax itself has a melting-point of 65°·5. When digested with ether in the cold, a portion is taken up by the ether, and a residue left of the melting-point of 67°; and, when dissolved in ether, if the etherial solution be filtered while warm from the first portions of the precipitate which crystallizes out, a substance may be obtained, of the melting-point of 72°, crystalline in appearance, hardly at all acted on by a solution of potash, but readily saponified by melted potash; resembling in short in all its properties the pure myricin. The products of the saponification of the wax itself closely resemble those of the impure myricin, and present similar difficulties of separation.

An acid may be obtained from it having the character of palmitic acid, and I have also procured from this wax the substance melissin, having a melting-point of 84°. The analysis VI. p. 279, was made from a preparation from Ceylon wax.

I will sum up the results of this investigation by giving a list of the principal substances of which an account has been given in this and the preceding papers. This table will exhibit, at one view, their relations to one another, and to the natural substances from the decomposition of which they are derived.

Cerotic acid [cerin] . . . . . $=C_{54}H_{54}O_4$.

Chlor-cerotic acid . . . . . $=C_{54}\begin{cases}H_{42}\\Cl_{12}\end{cases}O_4$.

Cerotic ether . . . . . $=C_{58}H_{58}O_4=\begin{cases}C_{54}H_{53}O_3.\\C_4\ H_5\ O.\end{cases}$

Chlor-cerotic ether . . . . . $=C_{58}\begin{cases}H_{46}\\Cl_{12}\end{cases}O_4=\begin{cases}C_{54}\begin{cases}H_{41}\\Cl_{12}\end{cases}O_3.\\C_4\ H_5\ O.\end{cases}$

Cerotin . . . . . $=C_{54}H_{56}O_2$.

Sulphate of oxide of cerotyle . . $=SO_3, C_{54}H_{55}O+HO$.

Chlor-cerotal . . . . . $=C_{54}\begin{cases}H_{41}\\Cl_{13}\end{cases}O_2$.

Ceroten [paraffin] . . . . . $=C_{54}H_{54}$.

Chlor-ceroten . . . . . $=\begin{cases}C_{54}\begin{cases}H_{35}\\Cl_{19}\end{cases}\\C_{54}\begin{cases}H_{33}\\Cl_{21}\end{cases}\\C_{54}\begin{cases}H_{32}\\Cl_{22}\end{cases}\end{cases}$

Chinese wax . . . . . $=C_{108}H_{108}O_4=\begin{cases}C_{54}H_{53}O_3\\C_{54}H_{55}O\end{cases}$

Melissin . . . . . $=C_{60}H_{62}O_2$.

Chlor-melal . . . . . $=C_{60}\begin{cases}H_{45\cdot5}\\Cl_{14\cdot5}\end{cases}O_2$.

Melissic acid . . . . . $=C_{60}H_{60}O_4$.

Melen [paraffin] . . . . . $=C_{60}H_{60}$.

Palmitic acid . . . . . $=C_{32}H_{32}O_4$.

Myricin (pure) . . . . . $=C_{92}H_{92}O_4=\begin{cases}C_{32}H_{31}O_3\\C_{60}H_{61}O\end{cases}$

I might add to this list the acid $C_{49}H_{49}O_4$, the constitution of which however, for the reasons I have given, I cannot consider to be made out with sufficient certainty.

We should naturally suspect some intimate chemical relation between wax and fat from their similar appearance and properties. This suspicion gave rise to the idea that wax was convertible into fat, and to the hypothesis that wax was to be regarded as the aldehyde of stearic acid, and was capable of passing into that substance by a simple process of oxidation, a view of its chemical nature entirely without foundation. From the preceding inquiry, we arrive however at the knowledge of a no less remarkable relation between these substances.

Margaric acid was recently the last of that singular series of acids of the type $C_mH_mO_4$, which commencing with formic acid comprehended acetic acid, the volatile acids of butter and the acid of spermaceti, and æthal was the last of the corresponding alcohols. In the wax acids and alcohols of which an account has been given in this and the preceding papers, we have bodies at the other extremity of the series standing in a similar relation to margaric acid and to æthal, as that in which acetic and butyric acid, and alcohol and potatoe oil stand to them at the commencement.

An intervening acid of the series, the acid $C_{44}\ H_{44}\ O_4$, has lately been discovered by Völcker* in the oil of the *Guilandina Moringa*, and the investigation of the numerous class of vegetable oils and waxes will doubtless afford other bodies of the group.

Notwithstanding the many different properties of these substances, we find their chemical analogies constant, and the mutual relation of the acid, the alcohol and the hydrocarbon, is the same between bodies containing sixty as between those containing only four equivalents of carbon. Through at least half the series, from thirty to sixty equivalents, the same physical type of fat prevails. As a fat is doubtless but a soft kind of wax, so may not alcohol be but a very fluid form of fat? Alcohol has not yet been solidified, but one cannot help suspecting that when solidified it will appear as a wax or fat.

Direct experiment has shown us that in the body of the bee sugar is converted into wax. A simple analysis of the two substances showed that the carbon and hydrogen were in the same ratio in both, and that the change could be effected by a simple deoxidation of the sugar. Of the way in which this change is effected we are ignorant. The true formulæ of these wax substances however shows that they belong to the very type of bodies which are the ordinary products of fermentation, and are connected with them by the strongest chemical analogies. A new mode of fermentation produced butyric acid out of sugar; might not another kind of fermentation produce wax?

Until we know the nature of the whole of the ingredients of the wax, it is useless to speculate on the law of such a change. Although the wax itself is no pure chemical substance, but a mixture of substances differing nearly three per cent. from one another in their amount of carbon, yet the analysis of the whole bees'-wax gives results showing in different specimens which I have examined, no difference of constitution which analysis can reach. This renders it probable that the action is definite, and that the sugar in all cases loses the same amount of oxygen, although the remaining elements may in different cases be differently grouped.

* Liebig's Annalen, vol. lxiv. p. 342.

IX. *On the Structure and Development of the Liver.* By C. H. Jones, *M.D.* *Communicated by Sir* Benjamin Collins Brodie, *Bart., F.R.S.*

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In venturing to offer a second communication to the Royal Society respecting the structure of the liver, I feel the rather anxious to do so, that I may have an opportunity of correcting an error and supplying a deficiency which existed in my previous paper. In the following observations I purpose to present some account of the structure of the liver examined in the ascending series of animals, and also to describe the several stages of its evolution in the embryo; in this way I trust I may be able to exhibit the characteristic structural features of the organ as it exists in Man and the higher animals, and also to determine the true place which ought to be assigned to it in a classification of the various glandular organs occurring in the same.

I am not aware that any detailed account of the structure of the liver has been recently published, except that by M. Natalis Guillot, which however, so far as I comprehend it, does not seem to be one that can be readily accepted; the idea that the minute biliary ducts and lymphatics originate together in a common net-work, is *à priori* improbable, and entirely opposed to conclusive evidence (as I think), which will be subsequently adduced. A very interesting paper on the structure and function of the liver has also appeared in the 4th volume of the Guy's Hospital Reports, from the pen of Dr. Williams; to his labours I shall several times have occasion to refer, but it will be seen that I differ from him in several particulars, especially respecting the importance of the basement or limitary membrane.

Commencing with the Bryozoon polype as the lowest individual in the animal series in which distinct traces of a liver have been discovered, we find that (according to Dr. Farre and Professor Owen) there are a number of follicles filled with a rich brown secretion, which open into a distinct compartment of the stomach; these doubtless constitute an hepatic organ.

In the Asterias, where though the several systems of organs are sketched out, they yet remain without any appearance of concentration and high individual development, there exists considerable doubt as to the part of the digestive system to which the function of a liver is to be attributed; the most usual opinion seems to regard the ramified appendage found at the dorsal aspect of the stomach as having this character. Dr. Williams, however, considers it more probable that the layer of cells, lining the dilated cæca of the prolongations of the digestive sac into the rays, discharges

the office of an hepatic apparatus, grounding his opinion on a comparison of the secreting elements in the two organs. My own examination of the dorsal appendage of the stomach does not lead me to agree with Dr. Williams, in excluding it from participating in the function of a liver; I find it to consist of cæcal follicles which present occasional bulgings; the diameter of these is about $\frac{1}{300}$th of an inch; they lie packed together in groups which are easily visible to the naked eye: in structure they consist of an homogeneous membrane, which encloses a mass of nuclear granules varying in size from $\frac{1}{3000}$–$\frac{1}{4000}$th of an inch; also granular globules, oval or circular transparent vesicles, yellow amorphous matter and oil-drops; between these and the contents of the gastric cæca I did not observe any remarkable difference; nevertheless, I do not mean to imply that the dorsal appendage serves, like the gastric cæca, for the reception of the chyle; but only that, admitting the secretory apparatus of the latter may possibly perform the office of producing bile, there seems no sufficient ground, from difference of structure, for supposing the former to be destined to a different purpose. I ought, however, to mention that in one or two specimens I examined, the structure of the appendage in question corresponded more nearly to the account given of it by Dr. Williams, but this appeared to me to be a deviation from the natural condition; nor indeed have I ever observed anything which could lead me to assent to the opinion that it represents a pancreas, an organ which first manifests itself unequivocally in the class of Fishes, and exhibits in the different genera so many beautiful stages of progressive complication.

In the Echinidæ, another kindred family, we find the alimentary canal, apparently of great simplicity, passing from the wonderful dental apparatus on one side, to the opposite pole of its elaborately constructed shell, performing only a few gyrations in its course; a few salivary cæca are described as entering the canal just before the œsophagus commences, but no mention is made of any structure serving the purpose of a liver; I have, however, found in the delicate walls of the intestine a layer of elongated follicles, much resembling the gastric tubuli of higher animals; these occupy the whole thickness of the intestinal wall, and are closely in apposition with each other; their colour is a deep yellow; they consist of nuclear granules and amorphous matter, with probably a yellow fluid; these materials seem to cohere simply together, and not to be contained in tubes of homogeneous membrane; the diameter of the follicle-shaped masses measures about $\frac{1}{143}$rd of an inch. If I be correct in supposing these follicles to secrete a fluid analogous to bile, it would give additional probability to the opinion of Dr. Williams respecting the function of the gastric cæca of the Asterias.

In the class of Annelides we may expect to find the hepatic apparatus in a very simple and primitive form, corresponding to the general configuration, which exhibits in so marked a manner the law of irrelative repetition. I shall describe the minute structure of the hepatic gland in three different instances, where its existence and position are satisfactorily ascertained, viz. in the Earthworm (*Lumbricus terrestris*), the

Leech (*Hirudo medicinalis*), and the Sandworm (*Arenicola Piscatorum*): among these we shall find some instructive and remarkable differences, the existence of which seems to indicate clearly that we must not rely too much on finding exact analogy of structure in any organ, even in the individuals of the same class. In the Earthworm, a thin yellow stratum is found applied over a great part of the outer surface of the intestine; it adheres intimately to it, and seems to be moulded on the convexities of the sacculi, not dipping deeply into the furrows by which they are separated. I have already described this yellow layer as consisting of dark-yellow masses, the majority of which cannot be seen to have an enveloping cell-membrane, while in others it is clearly perceptible though of extreme tenuity; the masses, with or without this envelope, are often seen elongated into a conical form, the apex directed towards the intestine, to which it often adheres pretty firmly; from this circumstance I formerly conceived that the biliary secretion was discharged by the bursting of the cells into the cavity of the intestine, each cell representing for a short space an attached follicle, and it is not impossible that this may be the case to some extent. But there is another mode in which the bile may come to exert its action on the chyle (if such indeed be necessary), viz. by the latter percolating the coats of the intestine, and consequently the layer of hepatic substance, before it is absorbed by the ramifications of the deep abdomino-dorsal vessel. This latter opinion seems to be strongly supported by the unquestionable condition of another part of the intestinal apparatus, known by the name of Typhlosole: this blind tube, which is beautifully plicated on its surface, extends along the dorsal aspect of the intestine from the gizzard nearly as far as the vent. In structure it consists of a strong homogeneous membrane, covered by a layer of ciliary epithelium, continuous with that which may be often seen on the rest of the intestinal surface, while internally it is lined by a thick stratum of biliary cells, almost precisely similar to those on the exterior of the intestine, and forming with them one continuous system. Now the typhlosole thus seems to be an inflection of the intestinal wall for the purpose of straining off the chyle from the coarse mass of the ingesta; and as it is certain that the biliary matter is not discharged from the cells which line it into the cavity of the intestine, it seems also probable, that at other parts of the intestinal surface the process is the same, the chyle transuding through them as it does through the membrane of the typhlosole.

In the Leech, as Prof. Owen has described it, the hepatic apparatus appears as a brown tissue, extending along the alimentary canal between the nervous cords and the mucous glands, and also upon the dorsal aspect of the anterior part of the cavity. It is composed, he states, of a congeries of elongated, convoluted and irregularly constricted follicles, which are united in groups by the confluence of their ducts into a single slender excretory tube: that this tissue, so well described, is truly the hepatic organ, cannot for a moment be doubted by any who have examined it with the microscope, the follicles being filled with minute spherules of a deep yellow colour, resembling precisely those which crowd the biliary cells in the Earthworm. Regarding it then

as the homologue of the hepatic apparatus we have just examined, I will enter on a rather more particular account of it, which will disclose I think some characters illustrative of hepatic structure in general. It may be described as consisting of two portions, one in which the constituent cells are free, entirely separate from each other, or only fused together in very small groups, and another in which the cells are completely fused together so as to represent either simple undilated tubes, or tubes irregularly constricted and bulged at intervals; the former is evidently that part of the organ where the production of fresh cells takes place; these are seen at first as granular cells with envelopes more or less distinct, but having only imperfect traces of nuclei; subsequently the characteristic biliary spherules appear in them, and at last fill them completely. Now the cells becoming developed in this manner (and they proceed exactly in a similar manner in the Earthworm) may either become fused together to constitute the tubes above described, or they may lose all trace of an enveloping membrane; their component spherules separating from each other, and the cell at last undergoing complete disintegration on the spot where it had been originally formed, while its elaborated contents are probably absorbed into the circulating stream, from whence as plasma they had proceeded. The contents of the tubes are doubtless poured into the intestine, while the débris of the free parenchymal cells are scattered throughout the surrounding tissue, and thus in this low Annelide we seem to recognise a division of the hepatic apparatus into two portions, similar to those which we shall shortly distinguish in the higher animals; these have probably separate functions, the tubular influencing by its secretion the intestine and its contents, while the parenchymal seems adapted to supply material directly for the use of the respiratory process.

In the Arenicola, the biliary cells are of a lighter colour than in the Leech or Earthworm; they are at first of a pale granular aspect and often exhibit nuclei; subsequently they acquire dark oily-looking contents, and often change their form, becoming remarkably elongated; their diameter varies from $\frac{1}{1000}$ to $\frac{1}{800}$th of an inch. If the outer surface of the intestine be examined with a low power, it is found to present the convexities of numerous short sacculi with intervening furrows, and to be perfectly even and defined, while the inner surface, on the contrary, has a flocculent, almost ragged appearance. This effect is due to the disposition of the cells, which form a moderately thick stratum, resting on an homogeneous membrane, the sole constituent of the intestinal wall; the cells are therefore actually in the cavity of the intestine, and in immediate contact with its contents; they contrast herein remarkably with those of the Earthworm, which we have seen to lie on the outside, for the cells of the Sandworm may be completely removed by washing the inner surface of the intestine, while those of the Earthworm can be thus detached only from the exterior.

These three instances from among the Annelides seem to exhibit the extreme varieties of condition which the hepatic apparatus is capable of assuming. In the Earthworm

it is wholly parenchymal, in the Sandworm wholly included by the intestinal membrane, while in the Leech it is of a mixed kind, consisting partly of tubes continuous with the intestine and partly of free parenchymal cells.

In the class of Insects, however widely the external form and the organs of the animal functions may vary, the hepatic apparatus preserves a remarkable uniformity, consisting of long slender cylindrical tubes varying in number from 4 to 200. Besides these tubes however, of whose truly hepatic character but little if any doubt can I think exist, there is another structure whose function is more doubtful; this is the so-called adipose tissue, which Mr. Newport believes to serve as a reservoir of nutriment, and also to fulfill in some way the office of lymphatic vessels, while Burmeister, Oken and Treviranus agree in regarding it as a portion of the hepatic apparatus. The opinion of these latter authorities is that to which I incline, for reasons which I will state, when I have given a description of the structure of the two systems of organs now adverted to. The hepatic tubes in the Blow-fly (*Musca vomitoria*) for a considerable part of their course present remarkable dilatations and constrictions; these are sometimes situated so as to face each other on opposite sides of the tube, at other parts they are placed more nearly alternate. Towards its commencement each tube is found to consist of a number of vesicles, which are arranged in a series so as to overlap each other; these vesicles are more or less perfectly fused together, their coalescence becoming more complete, and the margin more even as we advance towards the intestinal extremity of the tube. In many insects the walls of the tubes are perfectly even and present no bulgings or constrictions; even the terminal blind extremity is, I think, for the most part undilated. A most distinct and strongly-marked basement membrane, unsupported by any fibrous tissue, but often covered by minute ramifications of tracheæ, constitute the wall of the tubes; it is perfectly homogeneous, but presents nothing that I have ever seen corresponding to the germinal centres of Mr. Goodsir. On the interior of the basement membrane is a layer of granular matter of variable thickness; this is often of a deep yellow tint from the presence of biliary matter, or rendered opake for a greater or less extent by deposits of oily matter; in it are imbedded cells which are sometimes granular, and exhibit traces of nuclei, especially I think in their early stage, but mostly appear as large, delicate and quite transparent vesicles, which often escape from their bed of granular matter, and collect in the central canal, along which they glide rapidly when subjected to some degree of pressure; the diameter of these varies from $\frac{1}{1500}$ to $\frac{1}{600}$th of an inch; they are particularly remarkable from the circumstance that the secretion does not form *in* them, but under their influence probably in the surrounding granular matter. In most instances the layer of granular matter is of the same appearance in its whole thickness, but occasionally I have noticed that the stratum in contact with the homogeneous tunic was quite pale, while that bounding the central canal was more or less deeply tinged with yellow,—an interesting difference, which would seem to indicate a gradual evolution of the

material of the secretion from a previously formed granular blastema. The contents of the tubes are for the most part free in the cavity, and form a continuous layer along the wall; but sometimes, especially in the sacculated tubes of the fly, the cells and granular matter form separate masses, though these are not I think surrounded by envelopes*. The central canal varies much in width; sometimes it is very narrow or obstructed with oily or biliary matter, in other cases it is equal to half the diameter of the tube.

Directing our attention now to the so-called adipose tissue, which I will venture to name the parenchymal portion of the liver, it is to be observed that it varies a good deal both in its extent and in its general appearance in different instances; moreover, it is not always uniform throughout, but presents in different parts successive stages of development. In its simplest and primary state, it appears as a pale, granular and amorphous blastema, containing numerous large cells, the average diameter of which is $\frac{1}{750}$th of an inch; these are provided with a distinct envelope enclosing well-formed nuclei and granular matter of a yellowish tinge. Very numerous young cells are also present in the blastema; they are nucleated, and contain only pale granular matter; their diameter varies from $\frac{1}{3-1000}$ of an inch: lastly, oil-globules of a greenish yellow tinge, some large and others of minute size grouped together, are also found in this material; these seem clearly to arise from the blastema and not to be produced in cells, in which they are never found. Following on now the development of the parenchyma, we find that a mass of granular blastema, in which lie both young and fully-formed cells, assumes a definite form, its margin becomes even and invested by an homogeneous membrane; thus we have produced vesicles, or short tubes of various size and shape, which coalesce very irregularly with each other, and form a kind of coarse net-work. Occasionally the vesicles are found free and unconnected with others; sometimes also they are replaced more or less completely by long tubes, which might easily be mistaken for the hepatic tubes themselves. The contents of the vesicles or tubes vary a good deal; in Caterpillars they are wholly filled with large colourless oil-drops; in Butterflies, Maybugs, and some other insects, they contain a quantity of yellow biliary-looking matter, with various proportions of oily and granular matter. In some cases there is scarce any oily matter at all, but only granular, pale, or of a yellow tint. The primary cells may almost always be discerned in the interior of the vesicles, and upon them doubtless depend the persistent action of these structures. The interesting observation of Dr. Williams, respecting the termination of minute tracheary ramifications in the vesicles of moths and butterflies, I have been able in some measure to confirm: I cannot speak positively as to the tracheæ actually terminating in the vesicles, but I have seen them traversing an elongated vesicle from one end to the other, and thus holding nearly as intimate a relation to the contents as if they had actually terminated in them.

* Dr. Williams describes, as the constant arrangement of the contents of the tubes, large cells containing several secondary ones with granular and biliary matter.

As reasons for considering the tissue now described as part of the hepatic apparatus, I may mention,—1st, the close resemblance which often exists between the contents of the vesicles and those of the undoubted biliary tubes; in several instances, in fact, they have appeared to me identical. 2nd. The presence of distinctly yellow fluid, either infiltrating the granular contents of the vesicles, or united to oil-drops: this fluid may be reasonably supposed to be of the nature of bile. 3rd. The existence of much granular matter, shown by chemical tests to be of albuminous nature, of granular cells and nuclei in the vesicles, shows that they cannot be regarded as adipose tissue merely; nor should we expect to find this in such constancy and abundance in a class no higher than that of Insects. 4th. The oily matter which is found in them is usually not at all more abundant than in the liver of many higher animals; it exists also in a state of diffusion and combination with the granular matter, and its presence (since the tissue is surely not mere adipose) argues in some measure in favour of its hepatic character, it being well known how prone oily matter is to accumulate in hepatic structure. Were it proved by chemical tests that the yellow fluid, not unfrequently seen in the vesicles, is truly bile, no further argument would be needed; of this however I have been unable to obtain positive evidence; yet once I observed the yellow tint to be decidedly deepened by nitric acid, and from the action of this reagent and sulphuric acid in other instances, I am pretty well satisfied that albumen and oily matter are not the only contents of the vesicles.

The above arguments, though far from conclusive in favour of the opinion I have adopted, are not without weight; and I will only add, that if this view should prove correct, an analogy will obtain between the hepatic apparatus of the Leech and that now described as existing in Insects.

The structure of the liver in Crustaceans is so well known, and has been so well described, that I can add nothing to our knowledge respecting it; I would remark, however, that the large quantity of free oily matter which is found in the follicles of this class, must be regarded as an indication both of the feeble intensity of the respiratory process, and also that the secretion is not to be regarded as in any great degree excrementitious, since it would not then be found accumulating in such quantity. Before passing on, however, I must particularly allude to one interesting individual which is ranked with this class, the *Daphnia monoculus*: this little creature, from the transparency of its shell, and the simplicity of its organization, is a most favourable object for examination. Any one who would study the phenomena of muscular action in the living fibre to advantage, should not omit to examine a living specimen of this species; striped fibres exist in the larger ones, and the oscillating waves of contraction during the action of the fibre may be distinctly observed. The hepatic apparatus in the Daphnia I believe to consist of large cells whose diameter is about $\frac{1}{1-500}$th of an inch; these are chiefly aggregated round the intestine, but exist also in other parts; they have a distinct envelope, which encloses a transparent fluid and a large reddish yellow oil-drop, with one or two smaller ones; a nucleus is

sometimes discernible, especially when the secretion is in small quantity. These cells usually cohere together, yet retaining perfectly their separate outline; but sometimes the groups seem to become fused together, and to form a mass which is applied against the intestinal wall by a covering membrane. It seems quite certain that these cells are naked, and uncontained in any follicular offsets from the intestine; nor does there appear to be any provision for the transmission of the secretion thither; that they perform the function of a liver is I think highly probable; their appearance is generally such that one can scarcely hesitate to believe them to be agents of biliary secretion, and I cannot find any other apparatus which seems at all adapted to fulfill such an office. However, I must state that in some cases, especially when the Daphnia is laden with ova, these cells are almost devoid of their biliary-looking contents, and are much less manifest, though even then themselves or their nuclei are discernible more or less clearly. Still, as it is impossible to believe that they can be merely adipose tissue, I regard them as of hepatic nature, and would refer to them as an interesting example of a purely parenchymal hepatic organ.

As a specimen of the liver among Arachnidans, I believe that of the common house spider may be referred to; here the liver is very bulky indeed, occupying the greater part of the pouch-like abdomen; it is of a dull yellowish-white colour. In structure it consists of short cæcal follicles very closely set together, about $\frac{1}{84}$th of an inch in diameter; each of these is bounded by a well-marked limitary membrane, and stuffed with opake contents. The cells in the interior of the follicles measure about $\frac{1}{650}$th to $\frac{1}{800}$th of an inch; they have a very delicate envelope, and contain a quantity of opake oily-looking matter, with one or more granular but highly refracting vesicles; these are usually $\frac{1}{3-9000}$th of an inch in diameter, and appear subsequently, when set free, to undergo development into secreting cells. There is a good deal of free oily matter, but very little trace of bile pigment; the oily matter very often appears in the form of groups of small equal-sized vesicles $\frac{1}{6-5000}$th of an inch diameter, which almost seem to be enclosed in a definite envelope; this may be the commencement of the formation of nuclei or young cells under the favouring physical conditions which Ascherson has pointed out. The large size of the gland in this class is worth noticing, as well as the character of its contents, which consist principally of granular and oily matter.

Among the various families of the Mollusca, the follicular type of arrangement of the liver obtains, I believe, universally; the gland is remarkable in most cases for its large size, and frequently for the deep colour of its elaborated contents. In most cases the basement membrane of the follicles is extremely distinct, but occasionally it is very delicate, and even its existence has sometimes appeared to me doubtful; this led me originally to believe that the presence of the basement membrane was not essential to the act of secretion, but that its function was of a purely mechanical nature, supporting the secreting cells, and preserving constantly a passage to the excretory duct. Subsequent observations have quite confirmed this opinion, by

acquainting me with several instances where it is certainly absent from glandular structure; thus, in the pancreas of a pigeon, I recently ascertained the total absence of limitary membrane from the ultimate vesicles, the epithelium being arranged into follicle-shaped masses and tubes, the form of which was solely preserved by the cohesion of the particles together. In the Common Oyster (*Ostrea*) I noticed a similar instance; here, besides the well-known follicular liver, there is a thin lamella of reddish brown aspect, which lies between the mass of the liver and ovary and the adductor muscle in contact with the intestine, to which it adheres pretty firmly. In structure this consists of numerous cells aggregated together, and set as it were in an imperfectly fibrous tissue; these cells measure about $\frac{1}{1800}$th of an inch diameter, and are provided with an envelope enclosing biliary-looking granules, which sometimes fill them completely: nuclei are rarely to be seen in them, but free nuclei and young cells occur in the uniting tissue. Now these cells, which seem to belong to the hepatic apparatus, from the nature of their contents, are certainly not contained in follicles or any envelopes of limitary membrane, but lying interstitially fulfill without doubt a work of secretion. This view of the non-essentiality of the basement tissue was promulgated some years ago by the distinguished physiologist who gave the membrane its name and pointed out its wide-spread extent. To illustrate a condition of the liver not uncommon among Mollusca, I may mention the examination of a Fresh-water Snail (*Limnæus stagnalis*), which I made during the cold spring of last year; the organ was of very dark aspect and soft consistence, and its basement tissue was scarcely discernible; the secreting structure consisted of a large quantity of amorphous and biliary matter in a free state, with numerous cells, some of which exhibited an envelope, an interior nucleus, and granular or biliary contents; others were merely pale granular bodies: these cells seem to originate as delicate vesicles having a mean diameter of $\frac{1}{1000}$th of an inch; they acquire pale granular contents, and probably, at some period of their existence, nuclei; but from observation of this and several others of the lower animals, it has seemed difficult to believe that the energy of the cell is always dependent on the presence of a nucleus. In the higher animals I think this is certainly the case, and yet there would appear even in them to be an exception in the fat cells, which vary so much in size, that it seems certain their growth must continue long after the disappearance of the nucleus. A circumstance which was very striking in the examination of the liver of the Limnæus, was the prodigious quantity of dark biliary matter which was accumulated in it; in many parts it seemed as if the follicle-shaped masses were perfectly solid, converted, one might almost say, into biliary calculi. This state of the secreting apparatus must surely be conceived to imply a very tardy and imperfect discharge of the elaborated matter, and accords well with the imperfect character of the respiratory process. In a Sepia, one of the most highly organized among the Mollusca, the liver was still found to be of the follicular type; the terminal cavities were however very short and wide, and the limitary membrane by no means conspicuous: the contents were of

the usual kind, nuclei, cells, and abundant granular and oily matter, with some yellow masses which seemed to consist of concrete biliary substance.

In concluding this imperfect survey of the structure of the liver in the Invertebrate sub-kingdom, we may recapitulate by observing, that the tubular or follicular type of arrangement is that which generally prevails, and to which a tendency is almost invariably manifested; yet it has been shown to be probable in several instances, that a more or less considerable portion of the gland is in a condition which may be termed parenchymatous, the secreting structure being interstitially situated and not in connection with any excretory duct. In the next great division of animals at which we arrive, the hepatic apparatus is constantly distinguishable into a tubular and parenchymatous portion, but the latter now preponderates immensely, and the office of the tubular structure is confined to serving as an excretory duct; the evidence of this will clearly appear in the class of Fishes, which will next be investigated.

With the exception of the curious Lancelet (*Amphioxus*), the liver in all fishes may be stated, on the authority of Professor Owen, to be a parenchymatoid organ, provided with efferent hepatic ducts, and usually a gall-bladder and cystic duct, which pour the secretion into the duodenum. The blood-vessels entering the liver are now of different kinds, the portal vein or veins supplying altogether the parenchyma, while the hepatic artery is devoted principally to the ducts: this circumstance I think has not yet been fully accounted for; it will again attract our attention when we consider the manner in which the secretion is conveyed into the excretory ducts. Respecting the actual structure of the liver in Fishes, as in all Vertebrate animals, our knowledge has hitherto been very imperfect; for though, if I may use the term, the geography of the organ was perfectly set forth by Mr. Kiernan, yet since we have become acquainted with the actual agents in the process of secretion, the cells which elaborate the bile, and which constitute so very large a part of the organ, it has remained a complete mystery how the ultimate bile ducts were disposed with relation to the cells, and in what manner the secretion when formed was conveyed into the excretory passages. Before giving an account of the observations I have lately made respecting these points, I ought perhaps to mention that, according to Dr. Williams, the structural arrangement in fishes is nearly the same as in the molluscous tribes, the secreting cells being enclosed (he deems) in tubes of homogeneous membrane; this, however, is so completely contradicted by all that I have seen, that I shall do no more than thus cursorily allude to it. As it appears to me that in Fishes, the lowest of the Vertebrate classes, we find the liver at once assuming a very different type of arrangement from any which it has hitherto exhibited during our survey of the animal series, it is evidently a matter of great importance to determine, if possible, its real constitution, and to understand in what the essential change consists which has produced so marked a difference of form and character; fortunately this is rendered more practicable by the circumstance, that in the liver of fishes there is comparatively a small quantity of fibrous tissue (corresponding to the capsule of Glisson) diffused throughout

the gland, which therefore separates much more readily into its elementary parts: I have endeavoured to take advantage of this peculiarity to ascertain the actual condition of the ultimate hepatic ducts, and I trust that I have in some measure succeeded. Taking up a main branch of the duct, I find that by gentle traction and lacerating the surrounding parenchyma, I can isolate it with a multitude of ramifications without much difficulty, and after gentle washing remove it to a slip of glass. When now the specimen is spread out, and covered with a thin lamina, it can be conveniently examined: some of the branches, especially the larger ones, are evidently broken across, but a great number of the smaller ramifications do not appear to have suffered injury, and their characters may be thus described. They have a diameter of $\frac{1}{1000}$–$\frac{1}{800}$th of an inch at their origin from the trunks; they run a remarkably long course, giving off very few branches, and those for the most part at long intervals, though sometimes a group of minute branches arise close together; they taper slowly towards their extremities, which are found in various conditions, sometimes undoubtedly closed, with a defined rounded margin formed by homogeneous membrane; this however is rare; more usually the structure towards the extremity becomes less distinct, and it seems as if the duct gradually ceased. The larger ducts and the smaller at their commencement are invested by a thin layer of fibrous tissue; within this is a distinct basement membrane, which extends beyond the fibrous layer for a variable distance; sometimes it forms the rounded closed extremity of the duct, but mostly it becomes gradually faint, and can only be supposed to exist by the duct still exhibiting a well-defined margin; in many instances it certainly ceases some way from the terminal extremity. The contents of the ducts vary considerably; in the larger ones there may be either nuclei with granular matter forming an epithelial layer, or very delicate and pellucid vesicles; or the cavity may appear transparent, containing only some finely-mottled substance. Advancing to the smaller ducts, we find that they may likewise contain pellucid vesicles, so large as to occupy their whole cavity, but more frequently they are filled with nuclear granules and granular matter, the nuclei again being often very indistinct, so that there is scarce anything but granular matter to be detected; when they are in this condition the basement membrane also has generally disappeared, and the aspect of the duct, some way before its termination, is that of a tract of granular matter, which preserves accurately the tapering form and course of the original structure. In some cases the fibrous coat is prolonged further than usual, being continued as a filamentous expansion into the surrounding parenchyma beyond the terminal extremity of the duct. The foregoing description expresses the results at which I have arrived from numerous dissections of the liver in marine fishes; in the Perch (*Perca*), however, I have observed a condition of the ultimate ducts which differs from that now described in some respects, but resembles exactly that which is found in the mammalian liver; instead of appearing as minute cylinders of granular matter in which nuclei are scarcely perceptible, these ducts consist almost wholly of small nuclei set close together in a

scanty basis substance; they are quite devoid of basement membrane, and measure only $\frac{1}{8}$—$\frac{1}{3000}$th of an inch towards their terminal extremity. The mode which I have adopted of examining the ducts may be thought by some to be objectionable, on the ground that the extremities of the ducts must necessarily be lacerated; but I think a careful repetition of the process I have indicated, will satisfy the observer that it offers the most probable means for deciding the point in question; in fact I know of no other except injection which can be adopted, and I am satisfied that in this case the results of injection are not to be depended on. To one remarkable character of the ducts which has been mentioned I would ask especial attention, viz. the long course which the smaller ramifications take without giving off branches. I have observed one which ran $\frac{1}{50}$th of an inch without giving off any branches, and another which gave off only one branch in a course of $\frac{1}{10}$th of an inch. When we take into consideration this fact, and remember also how minute the terminal extremities of the ducts become, and that they are not unfrequently seen distinctly closed, it will appear quite certain that the ducts can have no actual connection with the surrounding parenchyma, so as to envelope and contain it in their terminal expansions, as some have supposed. The real relation of the ducts to the parenchyma is, I think, well shown by a peculiarity in their condition which I have noticed as of tolerably frequent occurrence; when this exists there are found in the parenchyma numerous masses of more or less deep biliary tinge, consisting apparently of large yellow granules enclosed in an envelope; when a duct is dissected out, these yellow masses are found adhering to it at various parts of its circumference; they are manifestly not in the cavity of the duct, but simply adherent to it. That these yellow masses originate in the secreting parenchyma is I think indubitable; they evidently consist of biliary matter; but this seems to have undergone some change, which has rendered it incapable of being readily absorbed by the ducts, and it thus remains on the exterior, indicating as it were the route it would normally pursue. I may here describe some remarkable structures which I have found in the liver of the Skate (*Raia Batis*), and respecting which I am rather in doubt whether they belong to the parenchyma or the ducts; these are vesicles of oval or subcircular form, measuring from $\frac{1}{750}$ to $\frac{1}{350}$th of an inch in diameter; they have a distinct envelope of homogeneous membrane, and a central cavity, which does not occupy more than half the diameter of the vesicle; the intervening substance between the wall and the cavity presents no distinct structure, but only some traces of concentric layers with a few nuclear corpuscles; the aspect of the vesicles is such as to imply that they are filled with some substance having a high refractive power, which is probably of oily nature, derived from that which is so abundantly diffused throughout the parenchyma. These vesicles are often found free, and unconnected with any other structures; they appear to be scattered throughout the parenchyma, but are sometimes at least involved in the fibrous sheaths of the ducts, so as to remain adherent to them; what their function may be is quite hypothetical; I can only conjecture that they may serve as reser-

voirs in part for the immense quantity of oily matter which is produced in the gland.

Turning now to the examination of the parenchyma of the liver in Fishes, it may be stated that it does not usually present a very marked division into lobules; this however is often indicated by the branching of the vessels, which do not run in interlobular fissures, such as exist in the liver of mammals. The term parenchyma is in the greater number of instances exactly expressive of the condition of the secreting portion of the hepatic apparatus in fishes, it is truly a substance poured among and upon the vessels and ducts, filling up completely the interstices between them, and forming together with them a solid mass: its actual state may vary a good deal; sometimes the greater part consists of perfect cells with distinct envelopes, and there is only a small quantity of free nuclei with granular and oily matter; at other times these are found to predominate, and but few perfect cells are to be detected; in this case however we very commonly observe a tendency to the formation of cells, the granular and oily matter being aggregated into cell-like masses, which either enclose or have enclosed at some time a nucleus. The masses of biliary granules, which I have already mentioned as adhering to the ducts, occur also free in the parenchyma; besides these, I have found in the Mackerel (*Scombrus Scomber*), and also in the Flounder (*Platessa flesus*), some peculiar cells which are perhaps of the same nature as the above-mentioned masses; they are larger than the secreting cells, of a perfectly circular form, and contain, within a well-marked envelope, from three to five oval or subcircular vesicles, which have often a nucleolar corpuscle in their centre, and possess a high refractive power; these cells lie free in the midst of the parenchyma, often forming small groups, which are imbedded in yellowish granular matter. The Flounder furnishes an exception in some measure to the rule, that the liver of fishes is not divided into lobules by fissures; the last twigs of the portal vein may be distinctly traced running in canals and short fissures in thin sections of the organ, these canals being much wider than the vessels contained in them; hence arises in certain conditions of the gland a remarkable disposition which is well worthy of notice. I have usually found the parenchyma of the liver in this fish of a yellowish white colour, and very opake from the great quantity of oily matter contained in it; but sometimes it is found much more transparent and of a redder tinge; in the former of these conditions the canals and fissures are comparatively empty, but in the latter they are filled with cells of about the same size as those of the parenchyma, but more opake and of a darker aspect from the oily nature of their contents. The interpretation which I would offer of this appearance is, that in this state the parietal cells of the canals and fissures do not readily dehisce and discharge their contents into the cavity, from whence they may be absorbed by the excretory ducts, but encroach upon and fill it up: we shall find in Mammalia instances of a somewhat similar occurrence, which appear to me particularly significant of the real constitution of the liver.

An important circumstance which cannot but arrest the attention in examining the livers of many fishes, especially those of the Skate and Cod, is the very large amount of oily matter which is manifestly in a free state; this, as we have on former occasions remarked, indicates a low intensity of the respiratory process, and also serves as a further proof of the correctness of the view here taken of the structure of the liver; for were this abundant oily matter free in the cavities of ducts, it should surely constitute in great part the contents of the gall-bladder, and so marked a difference would not exist between the dark-green fluid found in that receptacle, and the dead white of the adjacent parenchyma of the liver. Doubtless the excretory ducts separate, by a process of vital absorption, the bile from the oily matter in which they are bathed.

Among Reptiles I have examined the Common Snake (*Coluber natrix*), the Fresh-water Tortoise (*Emys europæa*), the Turtle (*Chelonia mydas*), the Newt (*Triton cristatus*), the Frog (*Rana temporaria*) and the Toad (*Bufo vulgaris*); in all these the liver is, κατ' ἐξοχήν, *a solid gland*, the capillary network extending equally in every direction: the coats of the capillaries are remarkably strong, and there can be no better opportunity than is here presented for examining their structure, which corresponds pretty closely with that figured by Henle. In most of these animals I have examined the ducts by dissecting them out in the way I have described, but by no means with such satisfactory results as I have obtained in fishes; this depends on the greater strength of the fibrous and other tissues composing the framework of the gland; however, in the Newt, Snake and Toad, I have been able to determine sufficient respecting the ducts, to satisfy me that they do not differ in any essential circumstance of structure or arrangement from those of fishes: thus it is quite certain that they bear only a small proportion to the mass of secreting parenchyma; they can be traced running a long course through it, and giving off very few branches, and in one or two instances they have clearly been seen to terminate by closed extremities. In structure they consist of a delicate epithelium with an investing layer of basement membrane, and often of fibrous tissue; these latter elements are often wanting in the smaller ducts, whose diameter varies from $\frac{3}{1000}$ to $\frac{1}{1000}$th of an inch; towards their terminal extremity also the tubular character is often completely lost, and the nuclei become indistinct, so that there remains only a tract of granulo-amorphous matter containing some minute oil-drops, which is soon lost in the surrounding parenchyma. This mode of termination in reptiles, as well as in fishes, is manifestly equivalent to terminating by a closed cæcal extremity, as far as regards a direct communication with the secreting parenchyma.

In none of the Reptilia that I have examined has there appeared any manifest tendency to a division of the parenchyma into lobules; it consists of delicate cells, free nuclei and granular, with a small proportion of oily matter: in the Snake these are arranged into masses of a definite form, which are disposed so as to represent short linear series coalescing to form a plexus, but in the others they merely occupy

and fill the interstices of the capillary network. Sometimes, as in the Toad and Snake, the greater part of the secreting structure is in the form of free nuclei and granular matter, with globules of secretion; in others, as the Tortoise, Frog and Newt, the perfect cells are more abundant; these are usually very delicate, appearing often like transparent spaces in the midst of the darker surrounding substance. Besides these, the ordinary elements of secreting structure, there are constantly found in all the animals I have mentioned, excepting the Snake, a great number of rather large dark yellowish corpuscles imbedded in the parenchyma; these in the Eft measured from $\frac{1}{1700}$ to $\frac{1}{850}$th of an inch; their form is usually circular or rather oval, but sometimes irregular when several have coalesced together; their margin is generally well-defined, they lie quite separate and unconnected with any other structures. I have not been able to determine exactly the mode of their production; sometimes it has appeared that a number of dark yellow granules were grouping themselves together to form a circular mass; they exhibited very active molecular motion, but were enclosed by no envelope; in other instances a delicate envelope is distinctly demonstrable, and once I have observed a mass of similar granules enclosed in a cell with envelope and nucleus. It seems on the whole probable that these dark corpuscles are produced in some way by the agency of cells, or perhaps by their nuclei alone, and it is scarcely doubtful that they must be considered as products of hepatic secretion, though an eminent chemist, whom I requested to examine them, was unable to discover in them the usual reactions of bile. These remarkable biliary concretions (for so I think we may call them), occurring in the liver of reptiles, are evidently identical in their nature and import with the yellow masses described in the liver of fishes; both are doubtless indications of imperfect excretory action; but the deep, almost black colour of the retained product in the reptile, and the great deficiency of oily matters in the gland, as compared with that of the fish, seem to lead to the conclusion, that a vicarious relation subsists between the colouring and fatty principles of the hepatic secretion; and this is borne out by the appearance which is presented in complete fatty degeneration of the human liver, compared with the same in a state of biliary congestion, the olive tint of the latter condition being replaced by the dull yellowish white of the former.

In Birds the liver is of a crisper texture, and much lighter colour than in most reptiles; it presents decided indications of a lobular arrangement, and in thin sections portal canals may be easily traced dividing into fissures. Its parenchyma varies in character; in a pigeon it was remarkably free from oil-globules, and consisted almost entirely of nuclei and granular matter, no perfect cells being discernible; in a swan (*Cygnus olor*) the secreting substance was of a dark greenish colour, owing to the presence of biliary matter, and chiefly disposed so as to form masses having a plexiform arrangement; in a duck (*Anas boschus*) the parenchyma was equally destitute of perfect cells, consisting entirely of nuclei and granular matter, with diffused oily particles; there were also numerous groups, as well as separate molecules of biliary matter, lying free in the parenchyma, and also in the fissures by which it was traversed.

These and several of the observations previously recorded render it, I think, certain that the formation of perfect cells is not necessary to the act of secretion, the nuclei alone are, doubtless, adequate to produce the necessary change in the exuded plasma; but if it be intended that the structure shall have a certain degree of permanence, then it is surrounded with an envelope, to isolate and confine in some measure the elaborated contents.

In the Pigeon, the ducts, when dissected out, were found to terminate very much in the same manner as they do in the liver of mammals; thus one small branch, $\frac{1}{1000}$th of an inch in diameter, appeared to terminate by a closed extremity; its walls consisted of nuclei set close together in an amorphous or finely granular basis-substance, and were not invested by any basement membrane; another branch, $\frac{1}{1000}$th of an inch in diameter, was seen in one part of its course to have a fine homogeneous tunic enclosing nuclei and granular matter; it terminated by losing its tubular character, and becoming resolved into a tract of very perfect and beautiful nuclei, which still retained the original form of the duct. In an owl I found one or two minute ducts which certainly seemed to terminate by closed extremities; they consisted principally of a finely granular substance containing small greenish yellow oil-drops; nuclei were seldom visible in them, and the basement membrane ceased before their terminal extremity. In an examination of a duck I obtained some very perfect specimens of the ultimate ducts; they were often long and slender, about $\frac{1}{3000}$th of an inch in diameter, and tapered very gradually to their extremity, which I think was in some instances certainly closed. In structure they resembled pretty nearly those of the Owl, appearing as cylindrical tracts of granular matter, but the nuclei in them were rather more distinct; in one of them which I have figured it is well seen how the basement tissue, distinct at one part, gradually fades away, and is lost as the duct diminishes toward its terminal extremity. From these observations there can be no doubt that the liver in birds is of the same type of structure as that which we have found to prevail in the two lower Vertebrate classes; we may remark again, in these animals of rapid circulation and active respiration, how the character of the oxidizing process affects the condition of the liver, a very small quantity of oily matter only existing in the parenchyma of the Pigeon, a bird of vigorous flight, while that of the stately and slow-moving Swan presents evident traces of retained secretion.

Lastly, we arrive at the Mammalia, in whom each of the glandular organs has a definite and unvarying type of structural arrangement. The tubes of the testis and kidney, the vesicles of the salivary and allied glands, are familiar to our thoughts and observations, but the well-defined and closely-crowded cells of the liver are yet scarcely acknowledged to constitute a parenchyma in the true acceptation of the word, several yet seeming to incline to the belief that they are contained in some way in terminal expansions of the ducts. In endeavouring to determine the long-mooted question, as to the mode in which the biliary ducts terminate, I have of course resorted to the method of injection, but except in the case of the Pig, I cannot think

that I have obtained any satisfactory results; a better prospect of success is I believe afforded by the mode of examination I have already described, in which actual recognition of structure, and not appearance only, is accepted as decisive evidence of the presence of the duct. Before proceeding to describe the results of my dissections of the ducts, I may state what appears to me to be an unexceptionable conclusion drawn from injection of the hepatic ducts in the Pig. Here, when a successful injection has been made in the double mode so successfully employed by Mr. Bowman, the lobules are seen to be definitely marked out by yellow lines or tracts, corresponding to and exactly occupying the "fissures" and "spaces." In several beautiful specimens thus prepared, the yellow line presents a most defined edge, and does not trench in the slightest degree upon the interior of the lobule; from this I cannot but conclude that no tubular duct penetrates the secreting structure; for were such the case, it is impossible that the injection should not, to some extent at least, have coloured the substance of the lobule. Hitherto I believe no particular description has been given of the structure of the minute branches of the hepatic duct; the larger ones are known to have a columnar epithelium resting on a subjacent basement membrane, which is strengthened by an investing layer of fibrous tissue. In the minute branches, which seem to be approaching their termination, and which sometimes can be isolated and examined in the most satisfactory manner, the epithelial particles are remarkably modified; they can scarcely be said to exist as separate individuals, but rather their nuclei, which are often large and distinct, are set close together in a subgranular or homogeneous basis-substance. In ducts where this condition of epithelium exists, there is seldom any distinct trace of basement membrane, the margin, though sufficiently even, yet exhibiting the bulging outlines of the component nuclei; still less is there any proper fibrous coat, though the ducts may be more or less involved in the filamentary expansions of the capsule of Glisson. Ducts of this character have usually a diameter of about $\frac{1}{1000}$th of an inch; they can be sometimes followed for a considerable distance, without being seen to give off any branches, or to diminish much in calibre. Their mode of termination is various; several have been distinctly seen to terminate by rounded and closed extremities, which have nearly the same diameter as the duct itself; others seem to lose their tubular character, their nuclei become less closely set together, and the uniting substance more faintly granular and indefinite; the duct in short gradually ceases, losing all determinate structure. In some of rather minute size, $\frac{1}{3-2000}$th of an inch in diameter, the exterior form remains distinct, but the canal is almost obliterated by the close approximation of the nuclei of the opposite walls. These structures now described I believe to be truly the terminal branches of the hepatic duct, from which they certainly originate; they seem gradually to lay aside the several component tissues of the larger ducts, the fibrous coat blending with the ramifications of Glisson's capsule, the basement membrane imperceptibly ceasing, and the epithelium being resolved at last into its simple fundamental nuclei. The above account has been taken chiefly from examination of

the liver of the Sheep, but in the Human subject and in the Pig I have made observations precisely similar.

In proceeding next to speak of the parenchyma of the liver in Mammals, I must recur to some points which I have dwelt on in a previous paper; several of these have been fully confirmed by my later observations, but in respect to one I have fallen into a considerable error. I described (as Mr. Bowman had previously done) the cells composing the lobules as arranged in long radiating series around the interlobular vein, the series however communicating with each other, and presenting a more or less decidedly plexiform arrangement: I observed the sides of the lobules to be invested by a membrane, which was continued across the floor of the fissure to line the side of the opposite lobule; this membrane, in the Rabbit and often in the Sheep, is truly homogeneous, and resembles exactly the basement tissue. I concluded it to be such, and believed that its presence afforded exact information of the mode in which the excretory duct terminated, which would thus appear to have expanded into the interlobular fissure. This view was further confirmed by the observation that the supposed basement membrane was often deficient, the marginal cells of the lobules being then irregularly prominent, and crowded with secretion globules, which appeared to be escaping in great numbers into the interlobular fissure. Subsequent examination has convinced me that the membrane investing the lobules is not really the basement tissue of the ducts, but a continuation of the capsule of Glisson; that however it is frequently of homogeneous texture, that it is also often absent, and that the marginal cells are then in the condition which may be termed active, are points which repeated examination has fully confirmed. Respecting the office of the interlobular fissure as a receptacle of the secretion, and *pro tanto* a portion of the duct, I hardly feel able to make confidently a general statement; it certainly is often seen crowded with globules of secretion, which have evidently been produced by the marginal cells of the lobules; at other times it appears empty, and the sides of the lobules are evenly lined by the investing membrane; not unfrequently (at least in the Sheep) the interlobular "fissure" has completely disappeared, and even the "space" become contracted, from the encroachment of the peripheral cells of the lobules, so that the mammalian liver has then assumed pretty nearly the condition of the undivided liver of Fishes:—a conclusion probably not far from the truth is, that the fissure may often serve as a receptacle for the secreted product, especially when the gland is in a high state of activity, but that under ordinary circumstances the removal of the secreted bile may be effected without its having been first received into the fissure. I may here record an observation which I made on the liver of a dog who died of granular disease of the kidneys. On the surface of a section of the organ there were seen a number of yellow ramifying lines or tracts, which were found to be occasioned by the accumulation of a quantity of yellowish material in the portal canals and fissures; this deposit undoubtedly was in part of biliary nature, and seemed to indicate that the absorbing action of the excretory ducts having been

checked, the secretion from the marginal cells of the lobules had accumulated in the fissures and canals, so as to produce the appearance described. Now that, which occurred to an extreme degree in this abnormal condition, may very probably take place to a lesser extent when the gland is in a healthy state, and there would seem in all cases to be a tendency to the occurrence of something similar. The theory, first suggested by Henle though not adopted by him, which I endeavoured to establish in my previous paper, respecting the mode in which the bile, elaborated by the cells in the interior of the lobule, is conveyed to the efferent duct on the exterior, viz. that it makes its way by transmission from cell to cell of a linear series, is I think somewhat confirmed by what I have occasionally observed respecting the disposition of the contents of the cells when arranged in a series; thus in one very perfect series of some considerable length, there was a distinct indication of a central canal extending throughout it, the granular matter in each lay in contact with the cell-wall, and the middle part of the cell was comparatively free. But though from the examination of certain livers we might be led to conclude that this mode of transmission was the only one in which the secretion of the cells was disposed of, yet instances are abundantly frequent which render it very probable that there is another and more direct mode in which the removal of the cell product, prone as it is to accumulate, is provided for. Now the following circumstance cannot I think but arrest the attention of all who are in the habit of examining the condition of hepatic structure, viz. that the quantity of *free* oily (perhaps also of biliary) matter varies very greatly; sometimes it is extremely abundant, at others scarce any is to be found: moreover it is manifest, that the product of secretion, while thus freely diffused, is just in the condition which renders it most exposed to the absorbing action of the circulating current. If the materials which serve as fuel for respiration are deficient in the blood, they may be readily absorbed into this fluid, as it percolates the lobular masses; and we may go further, and state that it is quite probable, that even while the biliary secretion is contained within the cell envelope, it is capable of being influenced by the state of the blood, so as to make its way by endosmosis, into the capillary streams, through the homogeneous membranes of the cell and blood-vessel. This view will not seem improbable, if we reflect on the simple constitution of the lobules, where a vast mass of naked cells is traversed by an exceedingly close capillary network; the cells and the blood are therefore brought into the closest relation possible, and it cannot but be on mere physical principles, that the contents of either should tend to intermingle. I may also remark, that this view supplies another reason besides that suggested by Mr. Simon for the absence of the basement membrane, viz. that not only the cells may be more freely exposed to the blood, but the blood also to the contents of the cells. The above suggestions, founded on structural characters, and on the varying conditions of the organ, harmonize well with the view which has lately received so much support from chemical inquiry, and which regards the bile as mainly intended to be absorbed and returned to the circulation; they may also help to explain why the bile has to pursue so tardy and indirect a course

before it can be received into the excretory duct; and lastly, if correct, they seem to establish that the liver is not widely removed from the class of glands destitute of efferent ducts, like them allowing a part of its elaborated products to return at once into the blood, from whence as plasma they had been derived. On a future occasion I trust to have the opportunity of examining more closely into the function of the liver. I may however at present remark, that if the capillary radicles of the portal vein absorb the saccharine and amylaceous matters from the intestines, it seems a wise provision that the blood so charged should percolate intimately a vast mass of cells, which may specially act on the newly-absorbed materials, retain them if they are in excess, convert them into biliary compounds, and again allow them to return, as needed, into the circulating current: this in fact is nearly the view proposed by Messrs. Bouchardat and Sandras. I have, lastly, to speak of the relation which the excretory ducts, before described, hold to the lobules of the parenchyma. As far as I can ascertain, they do not ramify very extensively, at least both in the Rabbit and Sheep many fissures appear quite destitute of them; in those where the margin is in an active state, the cells bare and discharging their contents, the absence of the duct may often I think be clearly determined; in others, where the margin is still covered by the investing membrane, it is more difficult to be certain, but generally I think it may be stated that they do not extend far beyond the "spaces;" here however I have in thin sections several times distinctly observed small ducts which terminated by closed extremities. In the portal canals small duct branches creep over the surface of the parietal lobules, and take up their elaborated products. In now endeavouring to determine the mode in which the biliary secretion formed exterior to the ducts arrives in their interior, we may recall with advantage the condition of the gland, as we found it to exist in the class of Fishes. Here it was seen that the excretory ducts, having coats of great tenuity and containing an active epithelium in their interior, ran a long course imbedded in the parenchyma, and bathed as it were in its copious secretion; moreover, coincidently with the assumption of the parenchymal form, we found that a separate vessel conveying a different kind of blood was appropriated to the nutrition of the hepatic duct and its branches, and it seems certain that a different material is formed in the interior of the excretory ducts, from that which is produced so abundantly around them. May it not therefore be considered as far from improbable, that the absorption of the secreted material is effected by an action of endosmotic character, not however one of mere physical kind, but vital, *i. e.* peculiar in its nature, and producing, I believe, at the same time a change more or less considerable in the fluid absorbed*? One further proof may be adduced in the class

* It may be observed, that, supposing the nucleus to be the essential agent in the elaboration of all secretions, the structural condition of the ultimate hepatic ducts is just that, which is best fitted for the office I believe them to fulfill, of eliminating from the product of the surrounding cells the bile itself: moreover this condition, which might in some measure be regarded as the natural result of their diminution in size, corresponds precisely with what seems to be required by the function ascribed to them,—an accordance such as is rarely seen except in the actual works of nature herself.

of Mammalia of the relation obtaining between the condition of the liver and the amount of respiration: the cells of a porpoise's liver were granular nucleated bodies, containing rarely a single oil-drop; nor was there more than a very minute quantity of oil in a free state throughout the gland,—a contrast most complete to the condition of the hepatic parenchyma in its co-inhabitants of the watery element, which respire not by lungs, but by branchiæ.

Development.

The development of the liver is described by Muller, with whom Valentin and others agree, as being formed on the fourth day of the incubation of the chick by a conical protrusion of the intestine, which soon acquires walls of considerable thickness, in the substance of which the ducts proceed to ramify; some of the "biliferous canals" however being apparently formed independently in the blastema itself. A different account is given by Reichert, who states that the rudimentary mass of the liver, as well as that of the pancreas, is merely a cellular growth from the surface of what he calls "membrana intermedia," which appears to be a layer of cells developed from the germinal disc, corresponding to the vascular and mucous layers, and destined to give origin to the vertebral, cutaneous, and sanguineous systems, and the digestive system, with the exception of its mucous membrane.

The following observations fully confirm the opinion of Reichert, so far as regards the independent origin of the liver; with respect however to some other points, the description which I venture to offer is in some degree different from any with which I am acquainted. On the morning of the fourth day I have found in the chick the chorda dorsalis and the rudiments of the vertebræ perfectly distinct; below these was a longitudinal fold with its convexity downwards, which was probably one of the "visceral laminæ;" towards the anterior part of this, and just behind the heart, there was a slight convex prominence, with a vascular free border, which appeared as a growth from the germinal membrane; this was probably the rudiment of the liver: no trace could be discerned of anything like an intestine. In another specimen about the same period, it was noticed that the developing vessels were very patchy and irregular, that in some parts there were merely spots or short streaks, and that though containing well-formed blood-globules, they did not appear to have distinct parietes. On the fifth morning the liver was quite distinct; it appeared as a somewhat reddish yellow mass situated just behind the heart, and presenting a free convex border below; above it ran the œsophagus curving forward and upwards, behind it was the stomach and the recently developed intestine. The border of the liver was pretty distinct in the whole of its circumference; a narrow space clearly intervened between it and the œsophagus and stomach, and there did not appear to be the slightest trace of the liver being derived from either of them; it seemed manifestly a separate and independent formation. The intestine distinguished from the œsophagus and stomach appears to be developed in the following manner, which I think has not

yet been correctly described. On the fifth day the constriction of the germinal membrane producing the vitelline duct was perfectly manifest, and the duct itself was filled with a dark oily mass contained in a distinct homogeneous membrane; this membrane constituting a complete sheath separated the cavity of the vitelline duct from that of the abdomen. It divided into two canals which took their course in nearly opposite directions; one ran forward and turned sharply upwards just behind the liver, and soon expanded into a somewhat dilated cavity. From this cavity several offsets were distinctly traced; one inclined upwards and a little forwards, widening as it proceeded, and soon opened into a transparent space with delicate but distinct parietes, which was evidently the stomach, as it opened at the other extremity into the œsophagus. A second offset, which was very distinct, passed upwards and backwards, and appeared to terminate in a cul de sac, but its destination could not be satisfactorily determined. From the anterior part of the intestinal cavity two less distinct offsets could be clearly traced proceeding into the liver; the upper one appeared to divide, but they could not be seen to ramify in the substance of the gland. The other canal proceeding from the vitelline duct ran backwards and upwards, and seemed clearly to be developing itself into the posterior part of the intestine, but was in a less advanced condition than that already described; at a subsequent period the two cæca, which afterwards attain so considerable a length, were seen sprouting out on either side from this portion of the intestine. Both these canals derived from the vitelline duct were filled with opake oily contents, so that their course was most satisfactorily traced; they also presented a distinct homogeneous enveloping membrane, and some trace of an external layer of granular aspect which afterwards becomes much more marked, and which seems to be derived from the germinal membrane itself.

Towards the close of the sixth day a change had taken place in the condition of the stomach; it no longer distinctly communicated with the developing intestine, but appeared as a thick well-defined mass, into which the canal of the œsophagus ran, and terminated by a marked cæcal dilatation. The anterior prolongation of the vitelline duct formed a considerable curve with the convexity upwards, then descending became indistinct, and terminated near the lower convex border of the liver. Shortly before its termination, it gave off towards the liver a bulging process, which exhibited some traces of dividing, but did not nearly reach the margin of the gland; a circumstance which greatly surprised me, as I had fully expected that these offsets from the intestine to the liver would have proceeded to develope themselves into the hepatic duct, and not have retrograded as they thus appeared to do. Towards the end of the seventh day the stomach is completed and overlapped by the two lobes of the liver; the duodenal loop is also distinctly formed; in its concavity there is seen a curved elongated tract, which stretches up to the hilus of the liver and is there somewhat enlarged; it consists of cells, granular matter and nuclei, with an oily blastema, and is clearly the rudiment of the pancreas; as yet it has no connection with the intes-

tinal cavity. The liver consists almost wholly of nuclei and diffused oily and granular matter, without perfect cells; the hepatic vein is very large and distinct, and arises from a regular network of capillaries in the substance of the organ; no trace of hepatic duct has yet made its appearance; the primitive offset from the intestine is still further removed from the parenchymatous mass. The prolongations of the vitelline duct (anterior and posterior) have now thick walls, and their cavities are beginning to be formed; they are almost shut off from the vitelline sac, the duct appearing only as a yellow streak.

Near the end of the eighth day there was no remarkable change in the pancreatic mass contained in the duodenal loop; it manifestly had no connection by ducts with the intestine, but was truly a parenchyma. Close to the liver on the right of the pancreas there were seen two tubes; the left one of these could be traced down for a considerable way, but was lost shortly before it arrived at the lower part of the pancreas; it consisted of a distinct but very fine homogeneous tunic enclosing delicate cells which formed a kind of epithelium. The other tube was distinct only near the liver, where it expanded into a pyriform sac (the gall-bladder), the cavity of which was distinct, lined by cells and invested by an homogeneous tunic; from this dilatation the rudiment of the cystic duct extended only a very little way towards the intestinal cavity. Near the termination of the ascending portion of the duodenal loop, at the part where the remains of the original offset were still perceptible, the wall of the intestine was deficient, and there existed a semitransparent space leading to the opake contents in the interior; but this was soon lost, and did not extend upwards to the ducts near the liver. At the beginning of the ninth day the gall-bladder was completely formed, and presented a very distinct cavity; its duct ran down and communicated with the intestine; in the upper part of its course it was extremely distinct, being provided with an homogeneous tunic, and containing an abundant epithelium; towards its lower part its structure is less distinct, but its course was clearly indicated by a transparent space, continuous with its canal, and extending through the intestinal wall. The hepatic duct lay by its side; it also was more developed above than below, and indeed could not be followed quite into the intestine though its future course was quite clear; at its upper part it ran up to the liver, and seemed just to penetrate it, but how it terminated in this direction could not be determined; its diameter was nearly uniform throughout, about $\frac{1}{1\,1\,1}$th of an inch. Both cystic and hepatic ducts were clearly seen to join the intestine *just at the part where the primitive offset from the anterior prolongation of the vitelline duct existed*; this was still represented by an accumulation of opake oily matter at the spot, but there was no bulging of the intestinal wall. The liver consisted of nuclei, granular and oily matter, and perfect cells; throughout its parenchyma there were many bright yellow particles of bile.

By the first hour of the tenth day, the ducts of the gall-bladder and liver were perfectly formed, and communicated with the intestine at the exact spot where an

opake mass still indicated the situation of the original offset from the intestine; the gall-bladder was quite visible to the naked eye, full of green bile, which was made, I think, by pressure to flow into the intestine. The pancreatic mass had now separated into two portions, skirting the contiguous margins of the duodenal loop; a rudiment also of its duct had begun to be developed; it appeared as a solid cylindrical tract of nuclei, continuous with a canal which entered the intestine together with the ducts from the liver. By the end of this day two pancreatic ducts were distinctly developed, one joining the hepatic just before its termination, the other entering the intestine together with the hepatic and cystic. I endeavoured to trace the further development of the hepatic duct with its branches in the substance of the liver, but without much success; it appeared to me however that they were first formed as solid tracts of nuclei, bearing some resemblance to the terminal ducts in the mature condition. The parenchyma of the liver now contained numerous particles of bright yellow or green bile; bile also filled the gall-bladder, and had flowed into the intestine.

The observations now related, warrant I think, the following conclusions:—

I. That the liver exists at one time as a parenchymatous mass, independent of any offset from the alimentary canal.

II. That though the first indications of the hepatic duct proceed from the intestine, yet these disappear, and are replaced by a separate and independent formation, which gradually developes itself further, both downwards to join the intestinal canal, and in the substance of the liver itself.

III. Biliary matter is formed in the hepatic parenchyma before its communication with the duodenum is freely established.

The following propositions may serve as a *resumé* of the principal conclusions to which we have been led during our survey of the various forms of hepatic structure.

The liver in all vertebrate animals may be regarded as consisting of a secreting parenchyma and excretory ducts.

The size of the excretory apparatus bears only a small proportion to that of the secretory.

These two portions of the liver are not continuous with each other, but disposed simply in a relation of juxtaposition.

The action of the liver seems to consist in the transmission of the bile as it is formed from cell to cell, till it arrives in the neighbourhood of the excretory ducts by which it is absorbed. This action is probably slow, and very liable to be interfered with, contrasting remarkably with that of the kidney, where a particular apparatus is added to ensure completeness and rapidity of action.

The secretion of the hepatic cells is very liable to be retained within the gland, either in the cells or in a free state.

This circumstance, as well as its structural peculiarities, seem to point out the liver as approximating to the class of ductless glands.

For the same reason it seems highly probable, that a part of the secretion of the cells is directly absorbed into the blood which traverses the lobules.

In a classification of the true glands the liver seems to occupy the lowest position, the highest being assigned to the permanently tubular, such as the kidney and testis.

From the condition of the secreting parenchyma in many instances, we learn that the secretory process by no means requires the formation of perfect cells in order to effect its peculiar changes; these may certainly occur in blastematous matter if a nucleus only be present.

The condition of the liver is in great measure dependent on the intensity of the respiratory process; its products being unused accumulate in the gland, often to a remarkable extent; its function is therefore not only vicarious of respiration, as formerly supposed, but preparatory, and to some extent subsidiary.

In concluding these inquiries, I cannot but acknowledge the kind assistance I have received from my friend Mr. P. Hewett, and also from Mr. H. Gray, who has several times aided me with his acute observation when a doubtful point was to be decided.

Appendix.

An objection may be urged against the account which I have given of the relation of the ultimate ducts to the cells of the liver, to the effect that they are in contact with these particles to so small an extent, that it is difficult to understand how the bile which they secrete should be received into the efferent ducts.

In answer to this I observe, first, that I believe much of the secretion of the cells is directly absorbed into the blood traversing the lobules.

Second, that I think it is by no means proved that the secretion of the cells is perfectly formed bile; in many instances it clearly is not: thus, in most fishes and in the fatty liver of the human subject, it is evident that the gorged parenchyma is full, not of bilious, but of oily matter, *out* of which however healthy bile is elaborated. I have carefully examined the cells of the human liver, in organs which were quite healthy, without finding any evidence of the presence of bile in their contents, though in congested livers the yellow molecules are often very distinct in the interior of the cells. In the Pig, Rabbit and Dog, the cells appear as pale granular bodies, and do not, that I can perceive, exhibit the least biliary tinge, even after the addition of nitric acid. In the Sheep there is generally a good deal of oily matter in the cells, but no biliary. I do not of course deny that bile is often formed in the cells, especially in states of congestion, but I conceive that in the perfectly healthy state, the complete elaboration of bile is effected principally by the nuclei of the terminal portions of the ducts.

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Explanation of the Plates.

PLATE IX.

Figs. 1, 2, 3, 4 represent cells from the liver of the Earthworm; (1) and (2) are elongated and filled with spherules of biliary matter; the envelope is prolonged into a tubular neck in (1.), diameter about $\frac{1}{1500}$th of an inch. (3.) has a delicate envelope, a nucleus, and some biliary contents, diameter $\frac{1}{600}$th of an inch. (4.) has a distinct envelope enclosing only pale granular matter, diameter $\frac{1}{857}$th of an inch.

Fig. 5 is an ideal section of an Earthworm, showing the relation of the typhlosole to the cavity of the intestine. The hepatic stratum on the exterior of the intestine is seen to be continuous with that in the typhlosole; *i* is the intestinal cavity; *t*, that of the typhlosole; *d*, the dorsal vessel in contact with *l*, *l*, *l*, the hepatic stratum.

Figs. 6, 7, 8, 9, 10 are taken from examination of the liver in the Leech. (6) represents two cells full of dark biliary contents cohering together. (7) a cell containing pale amorphous matter in which some biliary spherules have begun to appear, diameter $\frac{1}{857}$th of an inch. (8) is a cell containing only pale granular matter, no perceptible nucleus, diameter $\frac{1}{600}$th of an inch. (9) is a cell apparently dissolving; it has no envelope, and the biliary spherules are separating from each other, diameter $\frac{1}{1000}$th of an inch: in the centre there is a clear cavity which was observed in many others, as if the minute spherules were repelled outwards by some force. (10) a tube with several irregular bulgings, indicating the coalescence of the component cells; one cell is seen lying close by the side of the tube, but not yet united to it: the envelope of this tube was very faint in most parts; it almost resembled a solid tract of biliary matter.

Fig. 11. An hepatic tubule from the Blowfly (*Musca vomitoria*); it seems to be made up of a series of vesicles which overlie each other: the outlines of all are extremely distinct, and they all communicate together, being traversed by a central canal. It is not certain whether the first vesicle be truly the origin of the tube, or whether it has been detached from others with which it was in connection; there can, however, be no doubt that the specimen represents a tube very near, if not quite at its origin. The basement tissue of the tube was very conspicuous; it enclosed a great quantity of yellowish granular matter, in which were imbedded some transparent cells. In some parts of the central canal there were patches of opake oily matter; the diameter of the tube at its origin was $\frac{1}{300}$th of an inch; near its lower part $\frac{1}{143}$rd of an inch.

Fig. 12. Portion of hepatic tube from large white Moth; its diameter is $\frac{1}{350}$th of an

inch. It consists of an investing basement membrane, which is strongly marked; this supports a layer of pale granular matter, in which are imbedded several small transparent cells: the cavity of the tube is wide, and is occupied by numerous very delicate transparent cells, in the interstices of which are dark clusters of biliary granules. At *a*, two of the transparent cells are shown which had escaped from the tube; they are perfectly globular and contain only a limpid fluid; they are, I think, of constant occurrence in the hepatic tubes of insects.

Fig. 13. View of parenchymatous portion of the Blowfly's liver (or adipose tissue) in early stage of its formation. Several nucleated cells, diameter $\frac{1}{750}$th of an inch, containing granular matter, are seen lying in a pale granular blastema, along with several large oil-globules; a vesicle and tube are in process of formation from the blastema: these will include one or more nucleated cells. (*a*) is a nucleated cell from the blastema, diameter $\frac{1}{1400}$th of an inch, probably an early condition of the larger cells; (*b*) a fully-formed vesicle, diameter $\frac{1}{333}$nd of an inch, containing yellowish granular matter, and a nucleated cell.

Fig. 14. An elongated vesicle, $\frac{1}{14}$th of an inch long, $\frac{1}{111}$th of an inch wide at its middle, formed of an homogeneous membrane, enclosing dark oily contents; it is traversed in its whole length by a minute tracheary tube, which having become very fine issues from it at its upper extremity. (From brown Moth.)

PLATE X.

Figs. 15, 16, 17 from liver of Tench; they represent minute biliary ducts isolated from the parenchyma. (15) exhibits a delicate homogeneous tubular membrane, in the interior of which are several large delicate epithelial cells; the last of these lay so close to the extremity of the specimen, that it could not be determined whether the homogeneous membrane formed an actual cæcal termination; diameter $\frac{1}{3000}$th of an inch. (16) exhibits at its lower part the tubular character; towards its termination this is lost: it has the appearance of a tract of granular matter with even borders; several nuclei can be discerned in it; diameter $\frac{1}{1000}$th of an inch. (17) is a duct, $\frac{1}{750}$th of an inch diameter, consisting of nuclear granules and granular matter; its extremity is quite even, though it is difficult to determine whether it is completely invested by homogeneous membrane.

Fig. 18. A minute duct, probably terminal, from liver of Perch; its diameter is $\frac{1}{3000}$th of an inch: it consists of distinct nuclei, set as it were in a faintly mottled basis-substance.

Fig. 19. A group of peculiar cells from the parenchyma of liver of Mackerel; their diameter varies from $\frac{1}{1500}$ to $\frac{1}{1000}$th of an inch. They have a well-marked

envelope, and contain 3–5 circular or oval vesicles, which have somewhat the aspect of nuclei: the whole group of cells are imbedded in a collection of yellow granules of rather large size.

Fig. 20. One of the vesicles occurring in the parenchyma of the Skate's liver. The central cavity is shown with traces of surrounding laminæ concentric to the envelope, between which are some small vesicles: long diameter about $\frac{1}{350}$th of an inch; short $\frac{1}{400}$th of an inch.

Fig. 21. A rather large branch of the hepatic duct, diameter $\frac{1}{300}$th of an inch; its walls consist of homogeneous membrane with investing fibrous tissue; its cavity is filled with nuclear granules, finely granular matter and oil-globules; from its side there springs off a minute branch, which tapers evenly to its termination; its diameter is $\frac{1}{1500}$th of an inch; near its origin it has a distinct homogeneous tunic and a little fibrous investment; towards its extremity the homogeneous membrane can no longer be discerned, though the margins are even and the extremity rounded, and there is not the slightest appearance of its having suffered injury; it is I believe really a terminal branch becoming resolved, as many others are seen to be, into a delicate tract of granular matter; in some of these, as in fig. 16, nuclei are manifestly to be found; in others they are scarcely perceptible, or apparently absent. (From Flounder.)

Fig. 22. A large branch of the hepatic duct with several minute offsets; some of these, as well as the main branch, have pretty large yellow masses of biliary matter adhering to their exterior. The interior is filled with nuclear granules and granular matter. (From Tench.)

Fig. 23. A small, probably terminal, duct from liver of Grass Snake; it tapers from $\frac{1}{2000}$th to $\frac{1}{3000}$th of an inch; its extremity is rounded; no basement membrane can be discerned; it seems to consist of nuclei set in a finely mottled substance.

Fig. 24. A branch of the hepatic duct, with a minute lateral offset from liver of Duck. The parent branch has an homogeneous tunic with fibrous investment; on its interior there is a granular epithelial layer, in which nuclei are but faintly discernible; the offset is $\frac{1}{1250}$th of an inch in length, and $\frac{1}{3000}$th of an inch wide near its termination; it gives off no branches; in the greater part of its extent it has a tunic of basement membrane enclosing nuclei dispersed through granular matter; its terminal extremity was somewhat obscured, being in contact with some remains of parenchyma, but I am inclined to think it was closed; another specimen from the same was distinctly seen to terminate by a closed extremity.

Fig. 25. A minute duct from liver of Sheep perfectly isolated from other structures; its diameter is $\frac{1}{1000}$th of an inch, its length $\frac{1}{111}$th of an inch; it gives off no branches; its extremity appears to be closed; it has perfectly defined

margins, but no basement membrane can be detected by the most careful scrutiny; it consists of nuclei set in a partly granular, partly amorphous basis-substance; it issues from a quantity of fibrous tissue, which is seen at the upper part.

Fig. 26. View of a terminal duct from liver of Sheep lying in an interlobular fissure; it commences in a "space," and is seen clearly not to give off any branches in its whole extent, as it runs along the fissure; its extremity, I am nearly sure, was closed, though it was rather obscured by the investing membrane, under which it dipped as it approached the side of the lobule; the length of this duct was $\frac{1}{64}$th of an inch; its diameter, $\frac{1}{887}$th of an inch, was nearly uniform throughout; its walls were formed by nuclei of great distinctness, set close together in an amorphous basis-substance; no basement membrane could be positively said to exist; the hepatic duct had been injected, and a small mass of the colouring material was seen occupying the cavity of the minute duct where it lay in the interlobular "space."

Fig. 27. A minute duct, diameter $\frac{1}{1500}$th of an inch, from Human liver; it lies in a tract of fibrous tissue, from which however it is quite distinct; on its right margin there seems to be a delicate homogeneous membrane, but it ceases before the extremity; this is perfectly even, and evidently has not suffered injury; the duct chiefly consists of nuclei set in a subgranular basis-substance.

PLATE XI.

Fig. 28. A biliary duct from liver of Bullock with a lateral terminal branch; the walls chiefly consist of nuclei and granular matter, and are not invested by a distinct basement membrane except at *a*; the margin however is quite even; diameter of terminal extremity $\frac{1}{600}$th of an inch.

Fig. 29. Terminal duct from Human liver, diameter $\frac{1}{1500}$th of an inch; it is not invested by basement membrane, but consists of nuclei set in a faintly granular basis-substance; at its extremity it lies in contact with a group of hepatic cells.

Fig. 30. View of œsophagus, stomach, liver and intestine of Chick towards end of 6th day of incubation. Œs. œsophagus. S. stomach. L. liver; its two lobes are represented. V. sac, Vitelline sac. V. duct, Vitelline duct. A. anterior prolongation of vitelline duct. P. posterior prolongation of vitelline duct. H. offset to liver, primitive rudiment of hepatic duct.

X. *Minute structure of the Papillæ and Nerves of the Tongue of the Frog and Toad.*

By Augustus Waller, *M.D.* *Communicated by* Richard Owen, *Esq., F.R.S., &c.*

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THE attention of physiologists was first directed by me to the peculiar advantages possessed by the tongue of the living frog and other similar animals for microscopic investigation, in the year 1839. The extreme elasticity and transparency of this organ induced me to submit it to the microscope, principally with a view of examining the muscles during contraction. I communicated these experiments to M. Donne, who has mentioned my claim of priority in his Cours de Microscopie, p. 108, and they were first made public at the Société Philomatique, Aug. 17, 1839*. It will be unnecessary in a communication addressed to the Royal Society, to occupy the time of that learned body by recapitulating what is already known respecting the organ of taste. I shall therefore proceed at once to describe the results of my further researches on this organ, by which I have been enabled to determine the peculiar structure of the papillæ, and the ultimate termination of the nerves within them. In conclusion, I will point out the deductions which necessarily ensue with regard to the distinct nature of the functions of these organs in the act of taste.

Tongue of the Frog.

In this we find the same component parts as in the tongue of man. The principal points of difference are its smaller size, and the manner in which it is placed in the mouth. In other respects it presents the greatest analogy with that of the human subject. Its frame-work is composed of two muscles, the hyoglossus and the genioglossus. The hyoglossus arises from the inferior border of the body of hyoid bone and ascends to its superior border, the fibres diverging; afterwards it reflects backwards in the throat, the fibres forming a fan-like expansion. The genioglossus is a small, thick, triangular muscle, inserted by its base to the centre of the lower maxillary. The summit terminates near the inner third of the tongue in a tendinous extremity. These two muscles unite at an acute angle, and when at rest hang down the throat. The form of the tongue differs from that of other animals. The anterior extremity is broad, with a notch which divides it into two extremities or tubercles. The precise form of the extremity is best seen when compressed between two slips of glass. In a state of rest the extremity of the tongue hangs down the throat, where it serves as a valve to close the posterior nares in the act of swallowing the air for

* Minutes of which are to be seen in the Journal de l'Institut, p. 316, year 1839.

respiration. It likewise acts as an agent for seizing prey by being rapidly thrown out of the mouth, and enveloping the object to be laid hold of. In this act it is protruded from the mouth by turning round the lower jaw bone as a centre of rotation, the upper surface then becoming lowermost. The lingual arteries and veins are derived from the same trunks as in man, ascend the throat parallel to each other, and enter the tongue between the hyoglossus and the genioglossus. The nerves consist of two pairs, one direct from the brain, the other from the spinal marrow.

Mode of preparing the Frog's Tongue for examination.

In former experiments I confined the animal in a narrow bandage, which I rolled round it from the feet to the neck. In this state all movement of the limbs was completely prevented, while it was still able to carry on respiration. A piece of sheet cork, about the breadth and length of the animal, was then provided, and an opening made near one end of about the size of a shilling. After being secured to this cork, the tongue was turned out of the mouth and stretched over the opening by means of pins. But notwithstanding every care that could be taken it frequently happened that the experiment would be interrupted by the movement of the tongue, and its being torn from the pins. I am now able, by submitting the animal to the action of ether, to avoid these objections. For this purpose I find it most convenient to place the frog in a large wide-mouthed bottle, closed with a ground stopper, and containing ether. To prevent the contact of the ether with the animal's body, where it would produce inflammation, I keep the ether apart in a small phial, which is introduced into the bottle, so that in all the animal's movements it never is affected by the liquid. Placed in this kind of closed chamber, the frog becomes quickly narcotized by the ethereal atmosphere. The cessation of all motion shows the period when insensibility has taken place, and it may be withdrawn and the tongue expanded around the opening as before described. Usually the animals are completely insensible after about five minutes' exposure, and remain in that condition for upwards of half an hour. The insensibility may be prolonged to several hours by leaving them longer in the bottle. An exposure of half an hour generally renders them insensible for two hours. In this way we have the great advantage of avoiding all pain to the animal, independently of rendering the experiment more easy. I find the action of ether perfectly harmless to life, not having observed a single death in consequence of its action, even where it had been prolonged for several hours. A curious anomaly exists with regard to the full-grown female frogs; for I find in my experiments this winter that they are brought under its influence with much greater difficulty than the males and smaller animals, so much so, that after two hours' exposure they are less influenced than one of the latter after five minutes. The advantages of ether are so great that I have abandoned my former method, to which I shall only recur when I have to describe the difference in microscopic appearances in animals that have been etherized from those which have not.

Instead of sulphuric ether we may employ chloroform, muriatic ether, nitric ether, camphor, sulphuret of carbon, naphtha, alcohol, and various other volatile bodies. In some cases it will be found more advantageous to examine only a small portion of the tongue, which may be done by removing a small piece of the membrane with scissors, and interposing it between two slips of glass; this applies more particularly to those animals whose tongues are opake and not elastic.

Burdach has mentioned another method of examining the dead tongue by dipping it into a dilute solution of caustic potash, and then interposing it between the plates of the compressorium. As an auxiliary means, I may mention that by the application of a dilute solution of potash (about twenty parts water to one of liquor potassæ) we may also render the living tongue much more transparent. Another means of preparation is to keep the animal for several days after death, when the maceration of the organ in its own moisture, and the partial state of putrefaction, cause the detachment of the epithelial scales and the uncovering of the subjacent parts. Each of these means will be found to have, in certain cases, its peculiar advantage, and we cannot vary and multiply them too much, as in each case we view nature under a different aspect. As I have described on a former occasion* the principal phenomena connected with circulation in this organ, I will examine now those parts of the tongue which have reference to its sensorial functions of taste and of touch. The nerves which possess these powers are distributed and supported by the tegumentary membranes of the two sides of the tongue, our attention will therefore be directed to the various tissues of which these two membranes consist.

Vibratile Cilia and Rugæ.—The first parts which engage our attention, exclusively of the mucus on the surface, are the vibratile cilia of the tongue. The most active ciliary movement exists at the borders of the tongue. When a minute portion of the membrane is removed, anywhere near the edges and anterior extremity, we generally observe a most active movement at the borders of the fragment, and over its surface we find numerous channels running obliquely outwards and forwards, evidently corresponding to those rugæ we meet with on the human tongue in a similar situation. The ciliary filaments seen down these furrows meet at an obtuse angle, and exhibit a constant undulating movement transmitted downwards from one extremity to the other. Any small body coming into this channel is generally propelled quickly in one direction. Occasionally a succession of blood-particles are seen running down this channel which might easily be mistaken for blood circulating within a capillary. The appearance of the blood-particles viewed with a power of 400 diameters while beaten about by the ciliary filaments is sometimes very curious. The form of the vesicle is seen to vary in the most singular manner, sometimes dilated, sometimes compressed longitudinally or transversely, like a bladder partly filled with water when beaten about with rods. When the fragment has been freshly divided, a general tremor is observed at first, which arises from the irritation of the

* Phil. Mag. vol. xxix.

divided muscular fibres, and which ceases after a short time. The ciliary action, on the contrary, lasts a considerable time, and I have detected it two days after death in some instances. Even in a small detached fragment placed under the microscope I have known it to last for several hours, and it would probably continue still longer if kept moistened with saliva. After remaining about half an hour under examination the border of the tongue is seen to undergo an alteration. The particles of epithelium become uneven and gradually disaggregated. These entirely separate at some spots, and consequently numerous small, uneven cavities are formed along the borders. The surface of the fungiform papillæ is covered with cilia in active motion, while the conical papillæ by the side of them are entirely devoid of them.

The Conical Papillæ over the expanded tongue are found of various forms, conical or cylindrical; sometimes simple, sometimes compound, like so many conical projections seated on one body. We generally succeed in detecting an opening at the summit of each of them. The opening is either sharp at the edges or anal-like with circular lips. From above we see the commencement of a cavity lined with epithelium, which I have been able to see terminate in an infundibular canal extending towards the base of the papilla. These papillæ generally contain no vascular capillaries within them. When they do exist, they never ascend to the summit of the body, but form a bend or loop at about the half or lower third of the height of the cone.

Fungiform Papillæ.—These are easily distinguished from any other bodies on the surface of the tongue, by the existence within them of a coil of capillary vessels generally containing blood in a state of active circulation. They are always larger and redder than the conical papillæ; sometimes they present at the summit a red point of apparently extravasated blood, but which, when examined under a high power, is found to be merely an engorged vessel. A similar appearance on a larger scale is often seen on the fungiform papillæ in man. The fungiform papillæ consist of a circular zone of epithelian cells containing a central area filled with coils of capillary vessels, and with nerve-tubules ascending and terminating abruptly amongst them. When examined on an expanded tongue, we can form no accurate idea of the real elevation of these bodies, as they appear like discs adhering to the membrane subjacent, the stem not being visible; but if we remove a small fragment of the membrane and interpose it between glass, we find them standing out in relief at the borders, and of the same form as in other animals, viz. that of a flask or gourd. Some have a long pedicel, which becomes gradually narrower to its point of insertion to the membrane, where its thickness scarcely equals a fourth or fifth of that of the body. Others are with scarcely anything deserving the name of pedicel; and again some are compound, consisting of either of the above varieties, with the addition of one or two small conical papillæ joined to the external zone.

These varieties arise rather from their position on the tongue, than from any structural distinction. The degree of contraction of the pedicel and its height are connected with the height and size of the conical papillæ around them. Where these are long

and thickly studded, as at the dorsum and base of the tongue, the fungiform are likewise long and generally contracted. At the borders and at the tubercles, where the papillæ conicæ are short, the fungiform are thick, short, and surrounded by a thick protecting membrane of the same nature as in the papillæ conicæ, which in the lengthened fungiform papillæ is much less solid.

By the application of a minute quantity of solution of potash over a fungiform papilla, we sometimes observe a curious appearance. The external zone becomes separated from the central area by a deep fissure, and forms a kind of cup containing the blood-vessel, which appears like a spiral tube, and within this is seen the bundle of nerves. The circulation continues for some time in the papillary vessels even in this denuded state, then becomes languid, and finally ceases when coagulation of the blood takes place, unless the vessels burst at some point and extravasation of blood ensues, which is frequently the case, preventing any further observation.

The vessels of these papillæ are generally derived from arteries and veins, situated near the inferior surface of the tongue, from whence they ascend in a vertical direction until they reach the pedicel of the papilla. At this point the arterial and venous canals appear to be already reduced to the size of the ordinary capillary tubes, and they do not appear to undergo much, if any, further decrease of size in the capillary tuft at the summit. We might at first imagine that these coils are the continuation of one single tube, but such is not the case. They frequently communicate with each other, for when circulation is arrested or impeded in one loop, it often continues in the adjacent ones. They contain no valves such as are found at other parts of the tongue, for after any violent movement of the tongue the circulation often changes its direction, and what was at first an arterial capillary is afterwards found to convey the blood towards the heart like a vein.

Besides blood-vessels and nerves, we invariably discover in the interior of the fungiform papillæ numerous striated muscular fibres. They are derived from the superficial muscular layer, which exists beneath the basement membrane of the dorsum of the tongue, and appear to be one of the essential elements of the mucous tegument of that region. They run parallel with the vessels and nerves, to which they are external, and form a complete investment. After attaining nearly to the summit of the papilla, they curve inwards, and afterwards disappear in the surrounding tissues, apparently by losing their striæ and sarcolemma, which are their distinctive characters. This mode of termination of the fibres is deserving of attention, and is, I believe, the only instance in which the gradual transformation of the muscular element into any other tissue than the fibrous variety composing tendons has been discovered. I have before mentioned the ciliary motion on the surface of these papillæ, and its absence over the other papillæ which are destitute of muscular fibres. We are therefore led to the conclusion that ciliary and muscular power are more closely connected than is commonly imagined. The action of these fibres is to shorten the papilla, probably at the same time they may compress the vessels, regu-

lating to some extent the current of blood, and produce the turgescence of these papillæ which has been observed in the higher animals. The action of the cilia is very evident while under experiment. It conduces to clear away foreign bodies from the surface; to equalize the distribution of the sapid substance over them, and consequently over the nervous extremities; and to promote the removal of the epithelial scales which are constantly being shed.

Nerves of the Papillæ conicæ.—The epithelial scales veil these in general so completely that it is difficult to detect them. The application of the alkali which dissolves the scales, also disorganizes the nerves beneath. The plan which I find the most successful, is to macerate the part for an hour or two in saliva or water, when the increased transparency of the membrane renders the nerves more distinct. They are generally single, rarely two or three running together. Their course is irregular, wavy, with frequent simple loops, which enables them to present a much greater surface. In the tubule we frequently observe small granulations, but no white substance of SCHWANN is detected when perfectly fresh, although it frequently appears after the object has been kept for some time under examination. As a general rule in the conical papillæ, the nerve-tube runs close to the aperture of the papilla around which it forms loops, after which it runs away in a wavy direction. Often at each angle of the aperture is a nerve-loop of this kind formed by separate tubes, besides others which are seen running in a meandering course, and crossing the former in various directions. The space enclosed by these nervous loops is much darker than elsewhere, as if it contained some dark granular matter. The tubes never appear to terminate abruptly in free extremities. They are derived from trunks which give off at nearly regular intervals two or three tubules closely joined together, which afterwards subdivide in a manner more and more irregular, till they reach the state of single nerve-tubules.

It is evident that these are the nerves which convey the sensations of touch to the brain. The situation which they occupy at the base of the conical papillæ underneath the epithelian scales, can leave no doubt in this respect. It is true, that as at the base of the conical papillæ, and immediately beneath the epithelian scales, we find striated muscular fibres running in various directions, and some ascending into the interior of the body of the fungiform papillæ, it might be surmised whether these nerves are not destined to excite the contractile powers of these fibres. But their development, so utterly disproportioned to the office of stimulating a few muscular fibres, their mode of distribution in loops and convolutions, and their separation into single or double tubules, prevent our regarding them as muscular nerves. A curious point in reference to the nerves of touch, especially in the skin, are the fruitless attempts that have been made by numerous observers, to detect their ultimate terminations in the interior of the papillæ. On account of the impossibility of seeing the nerve-tubules within the papillæ, it has been imagined by some that they lose their external covering, and that they experience a gradual fusion with the

papillary structure which must effectually prevent our seeing them in these tissues. In my researches on the frog's tongue, I have never observed any alteration in the appearance of the tubules in support of this hypothesis, which I am therefore led to reject. In searching for the extremities of these nerves, which for brevity I will term the tactile nerves, in opposition to the others which are either gustatory or muscular, I experience considerable difficulty in detecting them until one or two simple tubes are seen, which being followed for some distance, serve as a clue to numerous other convolutions around them. By this means a spot which a moment before appeared covered with epithelium and destitute of nerves, is seen to be covered with abundant nerve-tubes distributed in the way I have mentioned. If such is the case with regard to the nearly transparent epithelium and papillæ of the frog, how much more so must it be the case in the papillæ of the skin, where observers have hitherto sought them, and where they are obliged to employ chemical agents to increase the transparency of this membrane!

To attain a view of their terminations, we are obliged to flatten the papillæ by compression. In this state we cannot determine to what height the nerves ascend within them. I have repeatedly in vain attempted to trace the nerve in the conical papillæ, seen in section at the borders of a fragment, while at the same time in an adjacent fungiform papilla I have obtained a perfect sight of the gustatory nerves. The farthest points to which I have followed them in these circumstances has been to their base, where the capillaries and the muscular fibres form a kind of basement structure. Here the nerves are found agglomerated together in knots, wherein the continuity of the tubules could not be traced. These knots were probably of a ganglionic nature. Over the vessels nerve-tubules of about one-third of the size of ordinary nerve-tubes were sometimes seen.

Nerves of the Fungiform Papillæ.—The papillary nerve may be seen at some distance before it reaches the pedicel, to form numerous waving incurvations, which appear to increase as it approaches it. Near the pedicel we usually perceive a kind of knot which contains numerous loops of the nerve. Before it reaches this knot, it is found to be composed of separate nerve-tubules, generally not more than five or six in number. If not sufficiently distinct, it may be rendered more so by a drop of alkali which dissolves the epithelium. In following the nerve to the pedicel, we perceive that it becomes darker, its fibres more confused, and occasionally with vesicular granules interposed between the tubules. When the expanded tongue is seen with a low power, the nervous knot at the pedicel is almost invariably detected by its dark-grey aspect and numerous loops. After forming this intricate arrangement, the nerve-tubules ascend into the interior of the papillæ, and expanding, become less dark. By the use of the compressorium and the alkali we are enabled to see their termination with ease. After nearly attaining the summit of the papillæ, we find some of the tubules to separate from the main body at an acute angle, proceeding until they reach some of the capillary vessels, where each tubule terminates abruptly, most

frequently with an irregular pointed extremity. The rest of the tubes still continue in close contact, and when they have attained the membrane of the area, end abruptly in an irregular manner, some in a point, some club-shaped, some in a spiral form, others like small funnels, but most often with a kind of concentric mouth.

In some papillæ the nerve-tubes keep close together until their termination, which takes place immediately at the surface of the area at its central point. Their open mouths are closely joined, and almost interlaced with one another. In others the terminations of the nerve are still more evident, for the tubes are seen expanding and crossing over each other so as to supply as equally as possible each of the areas enclosed by the capillary loops.

Nerves of the inferior surface of the tongue.—This surface presents neither conical nor fungiform papillæ. Its epithelium consists of flat nucleated scales, extremely thin. In a portion of this surface removed from the organ and interposed between glass, we may detect abundant convolutions of the nerves, similar in every respect to those found under the conical papillæ; they are very tortuous, form frequent loops, and are reduced to the state of nearly single tubes.

Mucous follicles.—These are seen over various parts of the upper surface interspersed among the papillæ. Their appearance at the surface is that of an anal opening generally closed during life, forming a slightly prominent tumid ring. After death, or when the membrane is much distended, the eye penetrates into their interior, where an active ciliary motion exists. When the surrounding membrane is denuded of its scales, we perceive around the opening two striated muscular fibres, forming a curve on each side, and performing the office of a sphincter. The follicle forms a bottle-shaped cavity, exactly like the small follicles over the skin of the frog, particularly near the anus. It is supplied by a capillary which runs close by it, without encircling or spreading over it. The follicular nerve consists of one or two tubules, and makes a single or double coil when it reaches the follicle.

In recapitulating these observations, we find in the frog an organ of taste similar in its general structure to that in man. At the upper surface are bodies corresponding to the conical and fungiform papillæ. At the inferior surface the membrane is smooth and without anything of the kind. We may therefore conclude that the upper surface has the faculty of taste and of touch, and that the under surface is merely tactile as in our own species. The fungiform papillæ consist of a membranous vesicle or utricule, containing coils of capillaries, numerous nerves, and muscular fibres, and probably lymphatics. Where do the nervous elements which these bodies contain extend themselves? We invariably find that they terminate at a part of the utricule where the membrane is so transparent that we may almost doubt whether they are surrounded at all by a membrane. This is the area which I term the *gustatory* or *neuro-vascular*, where the action of the nervous radicels is performed, which being conveyed to the brain excites an impression of taste. Another element observed in this area is the existence of numerous and intricate coils of capillary vessels

which surround the nervous extremities in all directions. We find also a zone or belt which encircles the gustatory area, and serves principally to protect and support it. Numerous muscular fibres are directed towards this zone, which will account for the partial erectility of the fungiform papillæ on certain occasions.

The gustatory area is placed sometimes at the extremity, at others at the sides of the papillæ. Whether this difference in situation be connected or not with any difference of sensation, it is impossible to determine; but in each case the gustatory area is placed conveniently near to any liquid spread over the tongue. It is further observed that the height of these papillæ varies with that of the surrounding papillæ conicæ. We are well-aware that it is only when in solution bodies can be tasted. The trituration of substances in the mouth has the effect of producing this wherever it is possible. When once in this condition, we cannot but admire the beautiful and simple structural arrangement by which taste is effected. The membrane of the gustatory area is so exceedingly thin that the transudation of any liquid in contact with it must be proportionately rapid. Accordingly, the open extremities of the nerves may be considered as immersed into the solution which it has to analyse, whether to reject, or to allow to pass on to the stomach. The mysterious action by which the material world comes in contact with mind, is being effected under our closest scrutiny. If we are ever to penetrate further into the arcana of life, may we not expect to do so by observing these phenomena while the vital powers are intact? While the nerve is being stimulated, the current within the capillary coil is continuing its course, and it requires but a slight acquaintance with the laws of imbibition to recognize that in this case the sapid solution must be rapidly eliminated, and that the nerve will consequently be free from its presence, and fresh to receive a new impression.

Tongue of the Toad.

Unlike what might have been expected from the habits of this animal, its organ of taste is less developed than that of the frog. The tongue is of a similar structure and form, and is covered with papillæ more minute and simple. The fungiform papillæ are less numerous. In the full-grown animal they present at their summit a circular capillary enclosing a fasciculus of nerve-tubes closely joined together. They also contain muscular fibres, and are provided with cilia.

The conical papillæ are less distinct, and are composed rather of folds or rugæ of the membrane than of separate bodies. On comparing a frog and toad of the same size, we find in the latter the papillæ much less developed. Taking, for example, a toad weighing five drachms, we find the epithelian scales over the dorsum very indistinct, and the surface finely granular. The fungiform papillæ are the $\frac{1}{350}$th of an inch in thickness, without any pedicel. The blood-vessels form two or three loops at the summit of the papilla. The nerve-tubules are much less numerous than those of the frog, less distinct and extremely varicose. The summit of the papilla consists

of a granular mass of a convex form, which, when viewed at the edges, appears surrounded by a fringe of epithelial cells, which is detached after a short maceration. The conical papillæ rarely exceed the $\frac{1}{1300}$th of an inch in height. Maceration causes them to swell considerably, so as to attain more than double their original size. The greatest increase is at their summit or free extremity, and the papilla then assumes a flask-like form. Some of the epithelial cells likewise attain an extraordinary increase of size by maceration.

Description of the Plate.

PLATE XII.

A portion of the frog's tongue removed from the border near one of the tubercles, as viewed with a magnifying power of 350 diameters, under a slight compression.

A. Fungiform papilla, projecting along the margin.

B. B. Conical papillæ, projecting along the margin.

C. Border of the tongue with vibratile cilia, seen in constant movement over the fungiform papillæ. The depression in the border is the commencement of a channel formed by the rugæ so numerous near the edges.

D. Small mucous follicle.

E. Apertures of the conical papillæ.

F. A capillary vessel containing blood-discs and corpuscles, ascending halfway up the papilla conica.

G. External zone of fungiform papilla, formed of epithelian cells which are much more indistinct and laminated over the neuro-vascular area.

H. Neuro-vascular area containing the capillary, the gustatory nerve, and striated muscular fibre.

I. The capillary ascending from the lower surface of the tongue towards the fungiform papilla, wherein it forms coils, making its exit in the same direction in which it entered. The vessel is represented in a state of engorgement, the globules compressed and indistinct.

J. The gustatory nerve, likewise derived from a branch near the inferior surface, entering the papilla between the capillary. Near its entrance it makes numerous wavy bends of a spiral form. The tubules become more distinct and diffused towards their extremities, where they appear to be composed of separate joints from the coagulation of the medulla of the tubule. Their extremities in this example appeared all of them to be slightly dilated, and with a dark point at their termination, giving them the appearance of ending in open mouths.

K. Striped muscular fibres ascending vertically into the papilla among the vessels and nerves, becoming indistinct near the summit.

L. Striated muscular fibres, forming hoops or circles beneath the mucous membrane; they are left plain to the right of the drawing to avoid complicating the figure.

M. Fibrous tissue of an elastic nature beneath the epithelium.

N. Tactile nerves forming a network over the muscular fibre. They merely attain the base of the conical papillæ.

O. An agglomeration of nerve-tubules. In this instance they appeared to be in part derived from the gustatory nerve before its ascent into the papilla.

XI. *On the Development and Homologies of the Carapace and Plastron of the Chelonian Reptiles.* *By Professor* Owen, *F.R.S. &c.*

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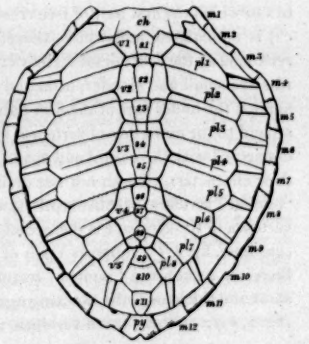
THOSE animals to which, in the manifold modifications of the organic framework, a portable dwelling or place of refuge has been given, in compensation of inferior powers of locomotion or other means of escape or defence, have always attracted especial attention; and of them the most remarkable, both for the complex construction of their abode as well as for their comparatively high organization, are the reptiles of the Chelonian order. The expanded thoracic-abdominal case, into which, in most Chelonians, the head, the tail and the four extremities can be withdrawn, and in some of the species, be there shut up by moveable doors closely fitting both the anterior and posterior apertures, as *e. g.* in the Box-tortoises (*Cinosternon*, *Cistudo*), has been the subject of many and excellent investigations; and not the least interesting result has been the discovery, that this seemingly special and anomalous superaddition to the ordinary vertebrate structure is due, in a great degree, to the modification of form and size, and, in a less degree, to a change of relative position, of ordinary elements of the vertebrate skeleton.

To ascertain the precise nature and extent of these modifications, in other words, to determine the homologies of the bony framework of the case in question, is the aim of the present communication.

The natural dwelling-chamber of the *Chelonia* consists chiefly, and in the marine species (*Chelone*) and mud-turtles (*Trionyx*) solely, of the floor and the roof: side-walls of variable extent are added in the freshwater species (*Emydians*) and land-tortoises (*Testudinians*). The whole consists chiefly of osseous 'plates' with superincumbent horny plates or 'scutes,' except in the soft or mud-tortoises (*Trionyx* and *Sphargis*), in which these latter are wanting. It is requisite briefly to allude to the well-known composition of the osseous framework of this chamber in order to define the terms by which certain parts will be adverted to in the course of the paper.

Fig. 1.

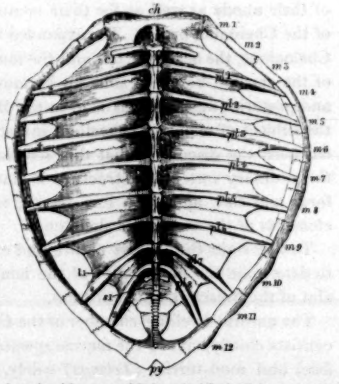


Carapace of the Loggerhead Turtle (*Chelone caouanna*).

The roof or 'carapace' (fig. 1) consists of a

'median' series of symmetrical plates (*ch*, s_1 to s_{11}), and of two 'lateral' series forming a pair (pl_1 to pl_8), the whole being surrounded by a circle of 'marginal' pieces (m_1 to *py*), completed anteriorly by *ch*, the first of the median series. Of the median series eight (s_1 to s_8) are attached to the spines of eight subjacent vertebræ: the lateral or parial plates (pl_1 to pl_8) are attached to, and more or less blended with, the ribs of the same vertebræ, and the ends of these ribs usually articulate by gomphosis with a corresponding number of the marginal pieces, of which, however, there may be from twenty-four to twenty-six, including the two median and symmetrical ones (*ch* and *py*). That these marginal pieces are the least essential parts of the carapace is shown not only by their inconstant number, but by their partial or total absence in some of the soft-turtles (*Gymnopus*, *Sphargis*).

Fig. 2.



Inner view of the carapace of the Loggerhead Turtle (*Chelone caouanna*).

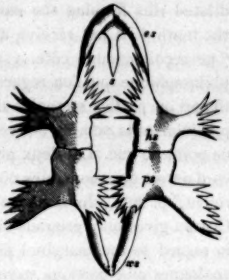
In the present communication the median pieces (s_1—s_{11}) are called the 'neural' plates; the lateral pieces (pl_1—pl_8) the 'costal' plates: the term 'marginal' is restricted to those peripheral pieces which form pairs (m_1 to m_{12}); the anterior symmetrical piece (*ch*), constant in all *Chelonia*, is called the 'nuchal' plate; the posterior symmetrical piece (*py*), which is wanting in all the *Trionycidæ*, is the 'pygal' plate. I enumerate the neural plates in the order in which they are numbered by Bojanus in the Tab. III. and IV. of his great work*. The neural arch connate with the first neural plate (s_1) is supported partly by the centrum of the vertebra to which the first pair of free ribs (fig. 2, c_1) is articulated, and which, therefore, is reckoned as the first dorsal vertebra: these ribs are small and slender, attached at both their extremities, the outer end abutting against the under part of the first pair of costal plates, which they help to sustain. The second to the ninth dorsal vertebræ inclusive, being those which are more immediately connected with the neural and costal plates, may be called 'vertebræ of the carapace:' their characters, though not less artificial than those which distinguish the 'dorsal' or 'lumbar' vertebræ of other reptiles, are much more marked and constant. The eighth vertebra of the carapace is succeeded by a vertebra, which in some species (*e. g. Chelone caouanna*, fig. 2, l_1) supports a pair of short ribs, in others (*Trionyx*) none, and which is therefore reckoned a 'lumbar' vertebra; this is followed by two other vertebræ, with short and thickened ribs, abutting against the iliac bones and representing the 'sacrum' (fig. 2, s_1): as these three vertebræ are not immediately united with the ninth, tenth

* Anatomia Testudinis Europææ. Fol. 1819–1821.

and eleventh 'neural plates,' they have less claim than the first dorsal vertebra to be regarded as entering into the composition of the carapace.

The 'plastron' (fig. 3) or floor of the thoracic-abdominal chamber consists in all *Chelonia* of nine pieces, for which the terms proposed by Geoffroy St. Hilaire may be retained, if used in an arbitrary sense and without implying assent to the hypothesis that first suggested them. The median and symmetrical piece of the plastron (fig. 3) is the 'entosternal' (*s*), the four pairs, counted from before backwards, are respectively, the 'episternals' (*es*), 'hyosternals' (*hs*), 'hyposternals' (*ps*), and 'xiphisternals' (*xs*).

Fig. 3.



Plastron of *Chelone caouanna*.

With regard to the ideas that have been entertained as to the homologies of the above-defined osseous pieces, it would be a parade of names without adequate gain to the discussion, to go further back than the first edition of the "Leçons d'Anatomie Comparée" (1799), in which Cuvier refers the chief part of the carapace, viz. the 'costal plates,' to eight pairs of dilated ribs*: the neural plates he describes as corresponding in number with the vertebræ of which they form part†: the marginal pieces and the parts ("plusieurs os") of the plastron are described arbitrarily and left undetermined.

Geoffroy St. Hilaire, entering into the question of their homologies in his memoir on the genus *Trionyx*, published in the year 1809, and adopting the Cuvierian idea that the carapace consisted of a development of dorsal vertebræ and vertebral ribs, argues that the plastron is a greatly expanded sternum, and that the marginal pieces are the cartilages of the ribs ossified, or 'sternal ribs' ("côtes sternales‡").

In the collection of memoirs forming the first edition of the 'Ossemens Fossiles,' Cuvier merely cites the opinion of Geoffroy, "Ces pièces que M. Geoffroy compare à la partie sternale ou cartilagineuse de nos côtes manquent aux *tortues molles*,"

* "La carapace des Tortues est formée par les dilatations de huit côtes ou batons osseux qui prennent naissance sur les unions des vertèbres et se terminent à un rebord que entourent toute la carapace." Tom. i. p. 211.

† "On remarque en dessus, le long de la partie moyenne, une rangée de petites plaques osseuses presque carrées, unies intimement entre elles par synarthrose, qui sont en même nombre que les vertèbres dont elles font partie." Ib. p. 211.

‡ "La différence dans le nombre des pièces du plastron et du sternum des oiseaux pouroit faire croire qu'il seroit entré dans le plastron des tortues des pièces étrangères à la composition d'un sternum, comme des côtes sternales; idée d'autant plus naturelle à admettre, que les parties latérales du plastron sont terminées par un certain nombre de digitations; cependant il n'en est rien. Les analogues des côtes sternales ne manquent point dans les tortues; elles existent dans ces pièces articulées dont j'ai parlé plus haut, et se voient à la suite des côtes vertébrales ou elles forment le bord des carapaces. Le plastron, ou le sternum des tortues s'attache sur ces côtes ou pièces sternales, en sorte qu'il ne manque rien d'essentiel dans le thorax des ces animaux."—*Annales du Muséum*, xiv. p. 7 (1809).

&c. (see the concluding memoir of tom. iv. 1812, p. 2). In the second edition of the 'Ossemens Fossiles' (tom. v. pt. 2. 1824), Cuvier, after remarking "that the marginal pieces do not correspond exactly to the vertebral ribs; that the first of the dilated ribs forming the carapace joins the third of the marginal pieces; and that the tenth does not receive any rib," observes, in reference to the latter expression, "ne reçoit aucune côte.... Ce ne pourroit donc être aussi que sous un point de vue philosophique que l'on regarderoit les pièces marginales comme réprésentant les cartilages ou parties sternales des côtes. Toutefois, comme il y en a onze, ce qui est précisément le nombre des vertèbres dorsales et lombaires, c'est un motif pour adopter ce point de vue. Les deux premières et les deux dernières servaient, comme on l'a dit en d'autres occasions, des côtes sternales auxquelles leurs côtes vertébrales manqueroient," p. 200. In the posthumous edition of the 'Leçons d'Anatomie Comparée,' Cuvier gives only grounds for rejecting, not any for adopting, the views of Geoffroy, in regard to the marginal pieces, and observes, "On a considéré ces pièces comme analogues aux portions sternales des côtes: il faut avouer au moins qu'elles ne leur répondent pas pour le nombre, et que dans les trionyx sur-tout, elles ne leur correspondent point pour la position. C'est à la troisième ou à la quatrième que commence leur engrenage avec les deux pièces moyennes du sternum; il finit à la huitième, mais dans les tortues de mer cette union n'a pas lieu." Tom. i. 1835.

Bojanus, who has given the most complete and masterly analysis of the emydian modification of the carapace and plastron, calls the neural plates 'processus spinosi vertebrarum dorsi,' the costal plates 'costæ,' and the bones of the plastron 'sternum:' he offers no homology of the 'marginal' plates, but retains for them the absolute names of 'ossa marginalia*.'

The eminent physiologists and comparative anatomists of our own country have not, however, partaken of this reserve of the great French master of the science, or of the celebrated German monographer, towards the Geoffroyan hypothesis of the marginal pieces. Dr. Roget, in his 'Bridgewater Treatise,' after a brief but clear summary of the general structure and uses of the carapace and plastron, says, "We find, however, on a more attentive examination, that all the bones composing the skeleton in other vertebrated animals exist also in the Tortoise; and that the bony case which envelopes all the other parts is really formed by an extension of the spinous processes of the vertebræ and ribs on the one side, and of the usual pieces which compose the sternum on the other." Vol. i. 1834, p. 464. The learned Professors of Comparative Anatomy in University College and King's College, London, have in like manner adopted absolutely the determinations of Geoffroy St. Hilaire, although the former admits that†, "Looking at the singular exterior of these tortoises, shielded in a solid case like a molluscous animal in its shell, we should scarcely expect to find that this dense osseous covering enveloping the whole body consists of the same

* *Op. cit.* p. 12.

† Lancet, February 8, 1834. See also Prof. Grant's 'Outlines of Comparative Anatomy,' 8vo, 1835, p. 82.

bones which compose the human skeleton:" and I must frankly avow that my expectation of such a discovery was so small as to beget neither surprise nor disappointment when the result of my researches into the development of the parts demonstrated on how superficial a view it had been entertained.

Professor Rymer Jones, in his beautifully illustrated 'General Outline of the Animal Kingdom,' adopting Cuvier's determination of the 'carapace' and Geoffroy's of the 'plastron,' observes, "The margin of the dorsal ribs is further enlarged by a third set of flat bones, apparently representing the sternal ribs of the Crocodile." 8vo, 1841, p. 553. In his article Reptilia, however, in Todd's Cyclopædia, Part 32, August, 1848,—the latest opinion on the subject which has been published,—the Professor affirms, "these marginal plates cannot be otherwise regarded than as the representatives of the sternal ribs of the Crocodiles and other Saurians." P. 266.

The German authors of standard works on comparative anatomy, with the exception of Meckel*, have manifested no such general acquiescence in the views of Geoffroy St. Hilaire, as that which characterises those of our countrymen above-cited. Carus, for example, originally regarded the immoveable 'costal plates' of the carapace as much-developed transverse processes, and the thorax of the Tortoise to be "only a more perfect development of the ribless and imperfect thorax of the Frog†;"—a view, however, in which his able English translator does not concur‡; and which Carus himself abandons in the second edition of his work. He there states that the remarkable and anomalous skeleton of the trunk of the *Chelonia* may be explained by recognising how certain plates belonging primitively to the dermal skeleton are applied or adapted to the vertebræ, the ribs and the sternum§: the idea, however, is neither explained in detail nor supported by any fact of development, but is rather obscured by such fancies, as that the bodies of the vertebræ of the carapace are not formed, as usual, on the under side, but on the upper side of the vertebral column in the place of the spinous processes, which Carus affirms not to exist‖.

Dr. Peters¶ adopts the view that the carapace includes dermal pieces besides the vertebræ and ribs; and that the plastron consists of a subdivided sternum enlarged by combination with ossified parts of the integument.

Professor Wagner has given us an opportunity of judging of the sense in which he

* System der Vergleichenden Anatomie, Zweiter Theil, Erst. Abth. pp. 407, 408.

† Introduction to Comparative Anatomy, by Gore, 8vo, 1827, p. 147.

‡ See the note at the same page, where the Geoffroyian interpretation is given, as more correct.

§ "Die Bildung des Rumpfskelett's nur dadurch erklärlich wird, das man einsehen lernt, wie durch Anbildung eigner, ursprünglich dem Hautskelet angehöriger Platten an Rückgrath, Rippen und Brustbein, die auf den ersten blick so sonderbar abweichende Bildung des Rücken- und Bauchschildes zu Stande kommt." Lehrbuch der Vergleich. Anatomie, 8vo. Bd. i. p. 164.

‖ "Am Rückenschilde das völlige Verwachsen der Wirbel, deren körper hier nicht wie gewöhnlich an der untern, sondern an der obern Wirbelseite, statt natürlich ganz fehlenden, und durch die darauf gelegten Knochenplatten des Hautskelets ersetzlen Dornfortsätze ausgebildet sind." Ib. p. 165.

¶ Observationes ad Anatomiam Cheloniorum, 1838.

understood Carus's idea, by the figure of the skeleton of a young Sea-turtle (*Chelone caouanna*), which he explains in his excellent 'Icones Zootomicæ,' fol. 1841*, Tab. XIV. fig. 12; where *a* are the ribs, *b* the vertebral bodies, *c* the neural arches (bogentheile), *d* the neural spines, and *e e* the median row of dermal bones ('mittlere Reihe der Hautknochen,' p. 17). Now these latter, in the figure, are six in number, extending from one end of the carapace to the other, whilst the subjacent neural spines agree in number with the vertebræ, of which there are twelve between the scapula and ilium. It is plain, therefore, that the horny 'vertebral scutes,' as they are called in Erpetology, are here the parts supposed to represent the dermo-skeleton, and that the bony 'neural plates' are regarded as the spinous processes, agreeably with the Cuvierian view.

Prof. Rathke† has recently propounded another modification of the combined dermo- and endo-skeletal hypothesis of Carus. Finding that there were no osseous plates developed independently in the corium and afterwards coalescing with the neural spines and ribs, as Carus and Wagner describe, he concludes that the carapace of the *Chelonia* is composed exclusively of endo-skeletal elements, but that the plastron as exclusively consists of exo-skeletal parts or dermal bones, in which category also he places the 'marginal pieces,' sufficiently proved by the *Trionyx* and *Sphargis* to be not essential to the composition of the carapace.

The special deductions by Rathke will be compared, in the sequel, with my own observations on the development of the carapace in the *Chelonia*; but it will be obvious, from the conflicting opinions on the nature and homologies of the chelonian skeleton, published within the last ten or fifteen years, that the question is far from having been satisfactorily settled; and that no one can be regarded as giving the requisite description of the carapace and plastron who merely adopts the determinations of Geoffroy, or Carus or Rathke, without first testing them by an appeal to nature, and assigning the grounds of his acceptance, rejection or modification of such determinations.

Commencing by the way of a comparison of the skeletons of fully-developed Vertebrata, and assuming for the purpose of such comparison that the thoracic-abdominal case is a modification of parts of the endo-skeleton, as Cuvier, Geoffroy and Meckel believed, I propose in the first place to test the homologies which have been generally accepted in this country, and of which, as regards the 'marginal plates,' so positive an opinion has been recently published.

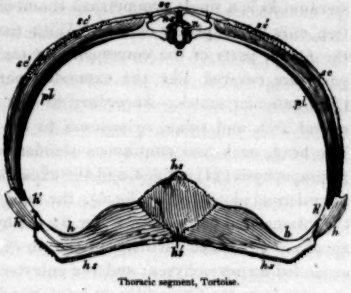
Geoffroy St. Hilaire was guided, as is well known, to his conclusions by the

* "Hier ist das skelet einer jungen Seeschildkröte (fig. xii) zu vergleichen, wo man sieht, dass Wirbelsäule, Rippen und Brustbein in ihrer ursprunglichen Anlage von dem eigentlichen Rücken- und Brüstschild ganz getrennt sind; das dieses eigentlich aus isolirten Verknöcherungen in der Haut entsteht, welche erst später mit Knochenskelet verwachsen." p. xii.

† Sur le dévelopment des Cheloniens. Annales des Sciences, Mars, 1846; and Ueber die Entwickelung der Schildkröten. 4to. 1848, p. 122.

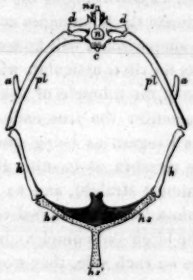
analogy of the thorax of the Bird: but they are not elucidated by any special descriptions or figures. They will be, perhaps, best understood by comparing the subjoined view of a segment of the thoracic-abdominal case of the Tortoise (fig. 4) with the corresponding view of the homologous segment in the Bird (fig. 5); in both of which *c* is the vertebral body or 'centrum,' *n* the neural arch, *ns* the neural spine, *pl* is the vertebral rib (pleurapophysis), *h* (the outer letter in fig. 4) is the ossified sternal rib (hæmapophysis), and *hs* the hæmal spine or 'sternum.' In this comparison it is supposed that the primitive median division of the sternum is retained in the cold-blooded reptile, and that the keel, or 'entosternal' piece (*hs'*), continues distinct, but is developed in breadth instead of depth. No one, however, has been able to adduce any example from the class of birds in which the lateral moieties of the broad sternum are developed each from four distinct centres, answering to the four lateral or parial pieces in the plastron of the *Chelonia* (*es*, *hs*, *ps* and *xs*, fig. 3).

Fig. 4.



Thoracic segment, Tortoise.

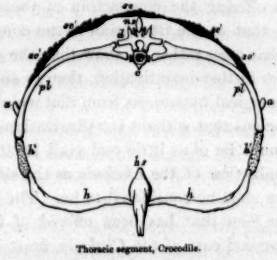
Fig. 5.



Thoracic segment, Bird.

The homologies of the carapace and plastron, regarded as developments of the endo-skeleton, appear, hitherto, not to have been elucidated by any other comparison, save that by Carus with the thorax of the Frog. Yet the chelonians have nearer affinities to the crocodiles than to either birds or batrachians; and a comparison of the thoracic-abdominal part of the skeleton of a crocodile appears to give correspondingly closer illustrations of the nature of the peculiarities of that in the Tortoise. In the subjoined view of the segment of the thorax of a crocodile (fig. 6), it will be observed

Fig. 6.



Thoracic segment, Crocodile.

that a distinct piece *h'* is interposed between the pleurapophysis (*pl*) and hæmapophysis (*h*), and it is less completely ossified than either of those elements. The sternum *hs* is a single symmetrical rhomboidal plate, of which a narrow median portion only is completely ossified. With the endo-skeletal segment is combined, in the figure, parts of the corresponding ossified segment of the exo-skeleton, which parts are covered, like the expanded parts of the carapace of the *Chelonia*, by thick cuticular scutes. According to this analogy, *c* being the centrum and *ns* the neural arch and spine, *sc* answers to the detached dermal bony plate *sc* in fig. 4. The head, neck and continuous slender part of the rib (*pl*, fig. 6) answers to the pleurapophysis (*pl*) in fig. 4, and the expanded plate (*sc'*, fig. 4) answers to the lateral bony dermal plates (*sc'*, *sc'*, fig. 6): the marginal plate *h*, *h'*, fig. 4, occupies the place of the intercalated costal piece *h'*, fig. 6: the hyosternal *h*, *hs*, fig. 4, answers to the hæmapophysis or ossified cartilage of the rib (*h*, fig. 6), the other parial pieces also being expanded hæmapophyses; and the entosternal *hs* (fig. 4) alone represents the simple sternum *hs* in the Crocodile: in brief, the figures within the segment fig. 4, indicate the homologies according to the Crocodile (fig. 6), those without or below the segment (fig. 4) indicate the homologies according to the Bird, fig. 5.

In this comparison it will be seen that the mesial end of the costal plate (*sc'*, fig. 4), which quits the rib to articulate with the vertebral plate (*sc*) in the Tortoise, is not the homologue of the tubercle of the rib which articulates with the diapophysis *d*, fig. 6, in the Crocodile: the true endo-skeletal pleurapophyses, or vertebral ribs, of the Chelonians I regard as being simple, and articulated by a head only to the central part of the vertebra, as in other Reptilia which have but one ventricle of the heart. They are almost straight, and so far resemble the free ribs (pleurapophyses), which project from a few of the dorsal vertebræ in the *Pipa* or Surinam Toad.

Were the large and complex abdominal hæmapophyses of the Plesiosaur (fig. 7, *h*) to coalesce on each side, they would form two lateral masses with their extremities projecting outwards and inwards, like the teeth of the hyosternals (*hs*) and hyposternals (*ps*) in the plastron of the *Turtles* and *Trionyces* (figg. 3 and 8).

In offering the comparison of the thoracic-abdominal segment of the Crocodile with that of the Chelonian to the consideration of Comparative Anatomists, my object has been rather to show that the subject admits of more than one view, and requires further investigation, than to substitute merely by such comparison a different homological hypothesis from that which has hitherto prevailed in this country; being conscious that without the illustrations of which such hypothesis may be susceptible, it would be of as little real avail in attaining to a true knowledge of the vertebrate organization of the *Chelonia* as the similarly unconfirmed view of Geoffroy St. Hilaire must be considered to be. The guide to our choice of either of these, or of any other view that has been offered of the nature and signification of the thoracic-abdominal case of the *Chelonia*, must be the light afforded by a true perception and explanation of the phenomena of its development.

The youngest Chelonian which I have had the opportunity of examining has been the embryo of the common Turtle (*Chelone Mydas*), not quite an inch in length (Pl. XIII. figs. 1, 2, 3). At this period the broadest part of the animal is the head, across the large prominent eye-balls. The neck is shorter than the head, the carapace is a long narrow ellipsoid, more convex than in the adult, defined by a feebly-indicated, thickened border: the region of the plastron (fig. 3) is flatter, perforated by the large vitelline duct and vessels (*u*). The scapular arch (fig. 2 *a*, $_{51}$, $_{52}$) divides the base of the neck from the fore-part of the carapace and plastron, and the anterior and posterior limbs present the simple form of undivided paddles, which they afterwards retain in this and other marine species. Although the ribs (fig. 1 *a*, d_1 to l_{10}) are visible through the integument of the back, and the slender entosternum (fig. 3 *a*, *s*) and two transverse linear rudiments of the plastron (*hs* and *ps*), on each side, are more obscurely seen beneath the integument of the abdomen, yet the corium covering these parts is thicker, and its texture denser than in the embryo of the lizard or that of the fowl of corresponding size and development; the general resemblance in the form of the body being very close at this period, to the bird, by reason of the normal proportions of the trunk and the shortness of the tail. The most advanced parts of the osseous system are plainly those which belong to the endo-skeleton, and which at this period deviate comparatively little from the normal type. As my present object relates to the thoracic-abdominal case, I shall confine my remarks chiefly to that part of the skeleton.

Ossific matter has begun to be deposited in the cartilaginous foundations of the neurapophyses (figs. n_1—n_{10}, 1 *a*, 2 *a*), and of the pleurapophyses (d_1—l_{10}), but not in the neural spines or the centrums.

Ten pairs of pleurapophyses (dorsal or vertebral ribs) have been established, much more nearly equal at the present than at a subsequent period; the first (d_1) and the two last (d_9 and l_{10}) being the shortest: all of them are simple, slender, cylindrical, slightly bent towards the ventral surface, terminating freely near the thickened border of the dermal basis of the carapace. The scapulæ (*ib.* 51) closely resemble the other pleurapophyses: it is impossible to mistake their general homology as the same elements of the vertebral segment: they are equally simple and cylindrical, and their ossification has made the same progress: but their position is more nearly vertical, with the upper end abutting against the fore-part of the first thoracic rib (d 1), and the lower end bent inwards towards the entosternum (*s*); the position is very similar to that which the scapula presents in the correspondingly developed embryo of the bird, in which, by a subsequent movement of backward rotation, the slender rib-like scapula comes to overlap the anterior thoracic ribs: but the primitive vertical position—the more normal position in relation to the archetypal skeleton—is retained throughout life in the *Chelonia* as in the *Monotremata*. In the region of the plastron the entosternum is represented by a slender median cartilage, pointed behind (fig. 3 *a*, *s*) the hyosternals (*hs*) by a pair of transverse cartilages, commencing near the

median line anterior to the umbilical aperture (*u*), and arching outwards, forwards, and slightly upwards to near the ends of the third pair of ribs: the hyposternals (*p s*) are represented by a similar transverse pair of slender cartilages, with a tendency to bifurcate at their extremities. The cartilaginous foundations of the episternals and xiphisternals have a not very definite linear form: the coracoids (fig. 1 *a*, ∞) are more plainly distinguishable; I at first mistook them for the episternals. The rudimental hyosternals and hyposternals at this period repeat the characters of the sternal or abdominal ribs (hæmapophyses) in the Crocodile; the entosternum represents the thoracic sternum of the Crocodile.

The thick and somewhat dense corium of the carapace, covering the rudiments of the neural arches and pleurapophyses, when examined under a power of 300 linear diameters, does not present exclusively the fine filamentous interlaced structure of cellular tissue in progress of condensation into derm, as in the embryo bird; but includes oblong nucleated cells, like those of cartilage, which along the middle line of the back are arranged in groups of linear series radiating from a centre corresponding with the point of convergence of each pair of neurapophyses, and connected with the extremities of those cartilages by a mass of cartilage-corpuscles holding the place of the neural spines.

The cartilage-corpuscles in the firm semiopake part of the corium covering the ribs, show traces of linear arrangement at right angles to the ribs, or in the axis of the carapace; especially near the proximal ends of the middle ribs. The thickened border which defines the carapace is formed almost entirely of oblong nucleated cartilage-corpuscles, pretty closely aggregated and without observable definite arrangement. The stratum of cartilage-corpuscles in the substance of the corium of the plastron is thinner than that of the carapace: something like a linear radiated arrangement of these may be discerned at the parts corresponding to near the mesial ends of the hyosternals and hyposternals; but they are for the most part irregularly and more thinly scattered in the fibrous tissue than on the carapace.

Homologically I conceive that this basis for future ossification, being situated in the substance of the skin, must be held to be the groundwork of a dermal skeleton; and that, whether ossification extends into such basis from the subjacent ossifying parts of the endo-skeleton, or whether it commences independently in the dermal cartilage, and afterwards unites with the deeper-seated bones, does not affect such homological relation: in other words, that a dermal bony scute, whether it be connate or become confluent* with a part of the endo-skeleton, is still essentially a dermal bone.

But although, with regard to most of the superadditions to the endo-skeletal basis of the carapace, I have not been able to distinguish a period of the development of an independent centre of ossification, yet the superadded parts, ossified from pre-

* I use the terms 'connate' and 'confluent' in the sense defined in my work on the 'Archetype of the Vertebrate Skeleton,' 8vo, p. 49.

existing subjacent vertebral elements, long retain a very peculiar and distinct character of osseous texture, well-displayed in the development of the carapace and plastron of the land-Tortoises, which I next proceed to describe.

Fig. 4 gives an outside view of the incipient carapace of a very young *Testudo indica*: fig. 5 shows an inside view of the same carapace, and figs. 6 and 7 similar views of the plastron of the same.

The carapace is not quite three inches in length. On removing, after maceration, the well-developed epidermal scutella, the following ossified parts were seen:—the nuchal (*ch*), the pygal (*py*), and ten intervening neural plates ($s\,1$ to $s\,10$); mostly of a subquadrate form, but of irregular size, and with rounded angles and ill-defined outlines; the tenth plate ($s\,10$) being insulated between the ninth ($s\,9$) and the pygal plate (*py*). On each side of the middle row of neural plates is a series of eight similarly-sized, triangular or rhomboidal plates (fig. 4, $pl\,1$—$pl\,8$), each of them marked on their outer surface with a triradiate linear impression formed by the junction of two costal scutella with one vertebral scutellum, or of one vertebral with two costal scutella; excepting the penultimate or seventh plate ($pl\,7$). Around the border of the carapace are eleven pairs of marginal plates ($m\,1$—$m\,11$), exclusive of the nuchal (*ch*) and pygal (*py*) plates. The wide interval between the marginal and the incipient costal plates is occupied by the corium and its stratum of cartilaginous cells, supported by the eight pairs of ribs of the carapace (fig. 5, $d\,2$—$d\,9$), by the first pair of short dorsal ribs ($d\,1$), by the pair of shorter lumbar ribs, and by the rib-like upper and outer extremities of the hyosternals (*hs*) and hyposternals (*ps*), which ascend beyond the marginal plates. The extremities of the ribs do not as yet join the marginal plates. The nuchal plate, the ninth and tenth neural plates, the pygal plate, and all the marginal plates are independent osseous developments in the substance of the derm: the other neural plates ($s\,1$—$s\,8$) are connate with the neural spines of the second to the ninth dorsal vertebræ inclusive, and the costal plates are similarly connate with the upper surface of the ribs of the same vertebræ at varying distances from their proximal ends. The first, second, fourth, sixth and eighth ribs of the carapace are continued from beneath the outer angles or apices of the corresponding costal plates ($pl\,1$, $pl\,2$, $pl\,4$, $pl\,6$, $pl\,8$), but the third, fifth and seventh ribs of the carapace are continued from beneath the middle of that side of the corresponding triangular costal plate which seems to form its base.

The neural plates, the costal plates, and the marginal plates, whether attached to vertebral elements or detached, are lodged in the substance of the derm, and form a stratum of bones superficial to the ossified parts of the endo-skeleton. A strong argument for regarding the costal plates as dermal ossifications rather than processes or continuations of the endo-skeletal elements, to which they are attached, may be drawn not only from the place of development of their cartilaginous basis or bed, but also from the period of their ossification; and their relative position to the ribs with which they are connate.

In the embryo *Testudo indica* the uniformly slender pleurapophyses (*d* 1—*d* 9, fig. 5) are ossified nearly throughout their whole length before the ossification of the costal plates, usually regarded as their expanded tubercles, commences: and the beginning of the superadded bone* is not at the same point in each rib, as might have been expected if it were the exogenous process called 'tubercle' of the rib. The costal plates are situated in the young *Testudo indica* (Plate XIII. figs. 4 and 5, pl_{1-8}) alternately nearer to and farther from the head of the rib; and their presence seems to be determined rather by the angle of union of the superincumbent vertebral scutella with the lateral or costal scutella, than by the necessity for additional strength in the articulation of the ribs with the spine. Ossification commences at the point from which the three impressions radiate, and as this point is nearer the median line at the median apex of the costal scutellum than at the lateral apex of the vertebral scutellum, the resulting plates of bone are alternately further from or nearer to the middle line; and the first, third and fifth costal plates have advanced along the proximal end of the rib so as to join the neural plates, whilst the second, fourth and sixth costal plates leave a portion of the proximal end of the rib uncovered and crossing the space between the incipient costal plate and the neural plate. In regarding these incipient ossifications, extending into the substance of the corium and receiving the impressions of the epidermal scutes as the developed 'tubercle' of the ribs, as Rathke has endeavoured to illustrate in Tab. III. figs. 11 (Tortoise), 12 and 13 (Chick) of his elaborate Monograph†, we are compelled to suppose that each successive rib in the Tortoise has a different position of its tubercle, which is alternately nearer and farther from the head, and that the neck of each successive rib is alternately long and short, which is contrary to all analogy furnished by those cold-blooded or warm-blooded *Vertebrata* that have unquestionably the exogenous process called 'tubercle' developed from the true neck of the rib.

When the partially ossified carapace of a young tortoise is dried, one cannot fail to be struck with the difference in the texture and external surface of the bones which unquestionably belong to the endo-skeletal vertebræ, and of those which, notwithstanding their connection with the neural spines and pleurapophyses, are developed in the fibrous substance of the corium. These nascent 'neural' and 'costal plates' of the carapace have a granular exterior and a coarse spongy texture, whilst the neural arches and pleurapophyses are compact, smooth, and with a polished external surface: the part of the pleurapophysis (Pl. XIII. fig. 5, *d* 2—*d* 9) which passes beneath and is attached to the under surface of the 'costal' plate (*pl* 1—*pl* 8), contrasts strongly with that superimposed dermal ossification.

The marginal plates (*m* 1—*m* 11) present the same rough, coarse, granular character as the neural and costal plates: they are in no way connected in their development

* This period, in its relation to the development of the neural arches and pleurapophyses, corresponds precisely with that at which the dermal plates of the Crocodile begin to be ossified.

† Ueber die Entwickelung der Schildkröten, 4to.

with the pleurapophyses, which do not yet reach them: their ossification has been governed by the presence of the marginal epidermal scutes, and, as in the case of the costal plates, by the points of junction of contiguous scutes; each marginal ossification is accordingly impressed by the lines indicating the junction of the marginal epidermal scutes with each other and, in the case of the middle ones, with the contiguous scutes of the plastron. The number of the marginal plates accords, moreover, with that of the marginal epidermal scutella, not with that of the ribs.

The plastron of the immature Tortoise (figs. 6 & 7) presents the same difference in the texture and surface of the endo-skeletal and exo-skeletal parts of the incipient bones as does the carapace: the triangular entosternal bone (*s*), the greater part of the episternals (*es*) and xiphisternals (*xs*), and a smaller proportion of the hyosternals (*hs*) and hyposternals (*ps*), are compact bone with a smooth shining free surface: the greater part of the broad hyosternal and hyposternal plates, the entire and even margins of which are encroaching on the central unossified space of the plastron, are subgranular, coarser and more opake than the slender endo-skeletal parts, which still retain much of the primitive rib-like form they presented in the fœtal *Chelone*, and are seen applied, as it were, to the inner (upper) surface of those dermal plates. The median extremities of the true endo-skeletal parts have begun to expand, and to shoot out the pointed rays of tooth-like processes which they retain in the *Trionyces* and the marine *Chelonia* (fig. 3). From the flattened and expanded inner and lower end of the hyosternal (fig. 7, *hs*) the main body of the bone arises and curves upwards, outwards and forwards, in the form of a long and slender rib, and applies itself to the inner and fore part of the first elongated pleurapophysis of the carapace, extending as far as the incipient dermo-costal plate; the rib-like part is represented detached from the rest of the hyosternal in fig. 5, *hs*. As the inner and lower toothed border of the endo-skeletal part of the hyosternal touches the outer border of the entosternal bone, the hæmal arch of the first segment of the thoracic-abdominal case (second vertebra of the back) is completed independently of the marginal pieces; and, in point of fact, the third and fourth marginal plates (fig. 8, *m*) are simply applied to the outer side of the hyosternal (*h*) where it bends upwards to join the first long pleurapophysis (*pl*) or rib of the carapace. The most obvious, and, I believe, the most natural explanation of this first complete segment of the thoracic-abdominal region of the young Tortoise, according to the typical vertebra, and the composition of the corresponding segment in the nearest allied *Vertebrata*, is—that the centrum (Pl. XIII. fig. 8, *c*), the neural arch (*ns*), and the pleurapophysis (*pl*), are the parts so indicated by the initial letters; that the hyosternals (*h*) are the hæmapophyses (sternal ribs or costal cartilages), and the entosternum (*hs*, *s*) is the 'hæmal spine' or sternum proper. The hyposternals in the young *Testudo* resemble the hyosternals, but are shorter; the slender rib-like portion which curves upwards and outwards applies itself to the back part of the extremity of the fifth rib of the carapace (fig. 5, *ps*), almost filling the interspace, for one-fourth of its length, between that rib and the next, and thus again forming the hæmal arch

of the segment without the intervention or aid of any of the marginal plates, the seventh of these being simply applied to the outside of the hyposternal, where its slender elongated extremity bends upwards to join the vertebral rib: and the only incomplete part of the arch is the unossified median space between the lower expanded and dentated ends of the hyposternals, between which the entosternal, or true sternal piece, does not extend backwards. So that the condition of this fifth segment of the thoracic-abdominal box, in the young Tortoise, repeats that of a posterior dorsal segment of a mammal or crocodile, in which the cartilages of the ribs, or abdominal ribs, do not reach the sternum; and the *Ornithorhynchus* offers a special resemblance to the Tortoise in the expansion of the semiossified hæmapophyses, or cartilages of its 'false ribs.' The xiphisternals, viewed in like manner as 'hæmapophyses', repeat the condition of those abdominal ones in the Crocodile and Plesiosaur which do not ascend so high as to join their pleurapophyses or vertebral ribs. The difference between the endo-skeletal and exo-skeletal portions of these elements of the plastron is as plain, and the contrast, indeed, is almost as great, in the young Tortoise as in the adult Trionyx, where the superadded ossification, at the expense of the dermal system, is characterized by the vermicular or punctate character of the exterior surface, a character common to the dermal ossified plates in the *Reptilia*, especially of the closely-allied Crocodilian order *.

The main purpose of the augmentation of the ordinary vertebral elements in the thoracic-abdominal region of the *Chelonia*, by the extension of ossification from them into the corium, and the consequent connation with those elements of dermal bony

* The distinction between the exo-skeletal and endo-skeletal parts of the plastron is so well-marked in the *Trionyx*, that the true explanation of the structure has forced itself, as it were, upon the authors who have given the most unqualified adhesion to the Cuvierian and Geoffroyian hypothesis. "Il est plus important de rappeler ici les caractères principaux—qui distinguent l'ordre des Tortues des trois groupes d'animaux rangés dans cette même classe des Reptiles; d'abord de tous les autres genres par la structure de leur squelette, dont les pièces qui constituent le tronc sont extérieures. Les vertèbres du dos, des lombes et du bassin étant soudées et solidement articulées, non seulement entre elles, mais avec les côtes et quelquefois le sternum, par de véritables sutures, ou unies par cette sorte d'engrenage que l'on nomme *synarthrose*; le tout forme ainsi une sorte de boîte,—une 'carapace'!—La partie inférieure du corps est également protégée par des pièces osseuses, correspondantes à un sternum, dont l'ensemble porte le nom de 'plastron.' "—Duméril and Bibron, Erpétologie Générale, 8vo, tom. i. p. 349, 1834.

The description of the carapace of a species of *Trionyx* is as follows:—"Cette espèce et la suivante sont les seules ou l'on ne compte sur le disque de la carapace que *sept callosités costales* de chaque côté d' l'épine dorsale, encore que ces deux espèces aient réellement huit paires de côtes comme tous les autres Gymnopodes. Cela vient de ce que chez le Gymnopode spinifère et chez le Gymnopode mutique il n'existe qu'une seule callosité pour les deux dernières côtes de chaque côte, tandis que dans les autres Gymnopodes les seize prolongemens costaux ont chacun leur callosité." The part here denominated 'callosity' is the connate dermal bone which is described in this memoir as the 'costal plate': but it is not more distinct in its mode of development, nor less connate with the subjacent rib in those *Trionyces*, which MM. Duméril and Bibron call 'Gymnopodes,' than it is in the ordinary Tortoises, Terrapenes and Turtles: only the superficial character of the superadded part is more distinct in the *Trionyces*: but it failed to draw the attention of the distinguished French erpetologists to a reconsideration of the homologies of the carapace which they had adopted.

plates, being the formation of a strong defensive abode, although the existence of the cuticular scutes, rather than of the parts of the endo-skeleton, determines the commencement of the ossification in the Tortoises, yet such ossification begins and proceeds in the dorsal and sternal integument of those *Chelonia*, e. g. *Trionyx* and *Sphargis*, that have no cuticular scutella, but a soft, uniform and lubricous integument. The influence, however, of this modification of the cuticular system on parts regarded as homologous with endo-skeletal elements in the ordinary *Chelonia*, is strikingly manifested, in the condition of the marginal plates and the variable proportions and even in the number of the ossified parts of the plastron, as *e. g.* in that of the *Trionyx granosus* (*Cryptopus*, D. & B.), in which a single dermal bony plate extends over the rudimentary hæmapophyses called hyosternals and hyposternals on each side. The cartilaginous matrix in the substance of the corium forming the margin of the carapace of *Sphargis* and *Trionyx*, receiving no stimulus from the presence of marginal scutella, is found to contain either mere scattered granules of ossific matter, as, *e. g.* in the *Trionyces* forming the genus *Gymnopus* of Dumeril and Bibron; or centres of ossification are established, as at the posterior part of the limb of the carapace in the species of *Cryptopus*, D. & B., which have no relation whatever with the presence, number or position of the vertebral ribs; and in these conditions of the border of the carapace we perceive a greater or less retention of the embryonic character noticed in the genus *Chelone*.

Summary.—The conclusions as to the homologies of the Chelonian carapace and plastron to which I have arrived from the observations above recorded and other details with which it has not been deemed necessary to encumber this communication, are as follow:—

1st. The centrum and the neural arch supporting the neural plate are parts the homologies of which admit of no question, and have given rise to none; but the neural plate itself is a dermal bone homologous with the median dermal scutes of the Crocodile's back-shield, but connate in some of the dorsal segments with the true neural spine in the *Chelonia*.

2nd. Only the free proximal and distal extremities of the costal plate and the narrow smooth prominent tract* continued from the one end to the other along the under part of the plate, represent the 'pleurapophysis' or vertebral rib; and this rib is accordingly simple, as in other reptiles with a tripartite heart: the external expanded portion which joins the neural plate is a dermal bone homologous with the medio-lateral dermal scutes in the Crocodiles, but connate with the pleurapophysis in the *Chelonia*.

3rd. The marginal plates are wholly dermal scutes; and even on the hypothesis that any of them, such for example as are penetrated by the free ends of the pleurapophyses, belong to the endo-skeleton, yet these answer rather to the accessory in-

* This part is well shown in the view of the inner surface of the carapace of the 'Water-Tortoise,' which Cheselden gives in the beautiful plate facing the 3rd Chapter of his magnificent 'Osteographia,' fol. 1733.

tervening pieces between the pleurapophyses and hæmapophyses of the Crocodilian thorax*, than to the hæmapophyses in their totality.

4th. The parial or lateral parts of the plastron, more especially the hyosternals and hyposternals, are the true 'hæmapophyses'; but in connation with dermal bony plates to which their characteristic breadth, especially in the land and freshwater Chelonians, is chiefly due. The entosternal, and perhaps the episternals, which repeat the transverse bar of the T-shaped sternum in *Lacertia* and *Monotremata*, are the sole parts of the plastron which can be referred to the 'sternum' in special homology and to the 'hæmal spine' of the typical vertebra in general homology.

Supplement.

The justly-merited reputation of Prof. Rathke as an embryologist, and the fact of his having deduced his views of the mixed nature of the thoracic-abdominal part of the skeleton of the Chelonian Reptiles from observation of its development, equally demand that his conclusions should not be abandoned without special grounds being assigned. Rathke concludes, as has been before stated, that the carapace belongs to the endo-skeleton exclusively†, and the plastron as exclusively to the exo-skeleton‡.

With regard to the carapace, he says, "The spinous processes are already developed from the second to the eighth dorsal vertebræ before the exclusion of the embryo, ... they remain pretty short, but contrary to the general laws of development of the vertebrate animals, they grow so much in breadth, that they form, after their ossification, a series of horizontal plates of moderate size§." He also takes occasion to confute the assertions of Carus, Wagner and Peters, that these plates are first developed independently in the derm and afterwards coalesce with the spines of the subjacent vertebræ. My observations concur with those of Rathke in regard to the fact that the neural plates, answering to the eight vertebræ of the carapace, are not developed independently of the neural spines, but are connate with, or ossified continuously from them‖. Nevertheless the position of the pre-existing fibro-cartilaginous matrix, and the distinctive character of the resulting ossification, appear to me to be stronger grounds for determining their dermal homology, than the mere fact of their connation in opposition to that view. The radius and ulna of the Frog are not only confluent but connate; *i. e.* they are progressively or continuously ossified from a

* These are well shown by Cheselden in the side view of the skeleton of the Crocodile, which forms the vignette of chapter iii., *op. cit.* They are not noticed in either of the editions of Cuvier's 'Ossemens Fossiles,' or of the 'Leçons d'Anatomie Comparée,' and are therefore unnoticed in most of our English compilations of Comparative Anatomy.

† Ueber die Entwickelung der Schildkröten. 4to, p. 105. ‡ Ib. p. 122–129.

§ *Loc. cit.*, and Annales des Sciences Naturelles, Mars, 1846.

‖ At least in the *Testudo* and *Chelone*. In some *Trionyces* ossification extends into the eighth neural plate from the median ends of the eighth costal plates, and in a new species which I have called *Trionyx planus* the same modification supersedes the seventh neural plate. These varieties are very significative of the dermal character of the neural plates.

common centre, yet their essential homologies are neither thereby destroyed nor much masked.

The unimportant, one might almost say accidental character of connation, in regard to the neural plates of the carapace, is shown by its absence in at least one-fourth of the series of those plates. Rathke admits that the first or nuchal plate, the ninth, tenth, eleventh and twelfth of the median series of plates of the carapace are developed from independent centres in the substance of the corium, and are therefore dermal bones. Now it is indisputable that these are the homotypes or serial homologues of the second to the eighth of the same median series of plates. The mere circumstance of connation of these plates with the closely subjacent vertebræ cannot make so essential a difference as is implied by their classification in a distinct skeleton-system from that to which their homotypes are admitted to belong.

With regard to the 'costal plates,' M. Rathke, after rightly stating that "all, or nearly all, of the ribs of the trunk-vertebræ are cylindrical until the exclusion of the embryo," proceeds to say, "they then begin to increase in breadth; ..... this increase commences at the place where the neck of the rib joins the body of the rib, and thence advances more or less towards the (distal) extremity: it becomes so considerable, that the bodies of all the ribs, by reason of the complete absence of intercostal muscles, come into contact on each side, either through their entire length, as in the genera *Emys*, *Terrapene*, *Testudo*, *Trionyx*, or nearly their whole length, as in *Chelone**."

The author appears to have traced, with great industry and perseverance, the development of the carapace in each of the genera which he cites in the above quotation: but the very general terms in which such development is described might have been suggested by a mere comparison of its results as they are manifested in the adult carapaces, except that in no species of *Trionyx* are the ribs united throughout their entire length: the extremity of the actual rib projects from the peripheral end of the superincumbent costal plate, even in the oldest specimens. M. Rathke proceeds:—"Soon after the eight pairs of ribs have begun to expand a process is sent off from above near the spinal column, which by its growth overlaps the few and slender dorsal muscles, and unites with the spinous process of the vertebræ†." Such a description of the development of the costal plates could be suggested, I believe, only by observation of a tolerably young specimen of *Chelone* or *Emys*. There is no mention of the development of the costal plates in the Tortoise (*Testudo*) by superposition of osseous matter upon the rib, the supporting part of which rib retains its normal form without expanding: there is no allusion to the alternately varying position of the superimposed dermal ossification in regard to the rib supporting it, nor to the relation of the incipient costal plates to the angles of union of the epidermal scutes. Perhaps these facts, so important in guiding us to the right homology of the costal plates of the carapace, were manifested in the young Tortoises examined by Prof. Rathke, though he has not described them: and yet it would be unfair, without

* *Loc. cit.* † *Loc. cit.*

stronger evidence, to suppose such defects in his description. They are, however, plainly demonstrated in my preparations, and are accurately represented in the figures 4 and 5 of Plate XIII. I entirely concur with Prof. Rathke in regarding the marginal plates as dermal bones: in every particular of developmental character they agree with one-fourth of the median series of bony plates of the carapace (neural plates); and in every respect, save connation with endo-skeletal bones, they agree with the rest of the median series and with all the costal series of bony plates of the carapace.

Finally, there remains for consideration Prof. Rathke's peculiar hypothesis of the nature of the plastron.

"The development of the plastron," he says, "takes place later than in Birds and Mammals: the cartilaginous basis consists of two pairs of very narrow and thin cartilages, one in front and the other behind the umbilical aperture: there is likewise formed a fifth azygous piece in most *Chelonians*" (he excepts *Sphargis* with a doubt), "between the two anterior parial pieces.... Subsequently there are developed in these different cartilages, more numerous osseous pieces, ordinarily, or perhaps always, nine in number. Their respective size varies greatly, according to the different species of Chelonians; for either they grow in such a way, the one in front of the other, that they meet by their corresponding borders in their whole length, so as to constitute a continuous plastron, or their growth is more restricted, and then they form a plastron open in the middle; or they constitute merely a narrow ring, as is probably the case in the *Sphargis**."

He alludes to other modifications of growth, which might equally have been suggested by the known varieties of the plastron in the adults of the different genera and species of *Chelonia*; *e.g.* where it extends forwards beyond the neck, and backwards beyond the tail, which he thinks may probably depend upon the presence of an inferior fold of chorion existing in front of the fore-feet, and of another inferior fold behind the hind-feet: although he admits that where, as in the *Trionyx*, such folds occur, they are not occupied by the plastron, which fact invalidates the hypothesis.

My observations do not agree with those of Rathke, which have led him to ascribe the eight parial pieces of the plastron to the development of as numerous osseous pieces in the two pairs of primitive slender cartilages. I find no other ossification set up on the anterior pair of those cartilages than that which results in the formation of the hyosternals; and no other in the posterior pair than that which results in the formation of the hyposternals. The episternals unquestionably have independent cartilages, and so I believe have the xiphisternals, though I have failed to get so clear a demonstration of the latter.

The primitive cartilages of the true sternum (entosternal) and the thoracic-abdominal hæmapophyses (hyosternals and hyposternals) are distinct from, and deeper-seated than, the thin stratum of cartilage-cells which pervades and thickens the ventral fibrous integument. I am unwilling to suppose that Rathke could have ever wit-

* *Loc. cit.*

nessed the marked distinction between the ossification of these endo-skeletal parts and the superadded dermal ossified layer, and have made no mention of it. He, in fact, maintains his opinion, "that the plastron is nothing else but a part of the dermo-skeleton, and that it has nothing in common, in an anatomical point of view, with the sternum of other animals*," thus diverging to an opposite extreme from that of the Geoffroyan hypothesis, although rather by arguments drawn from the relative position of other parts of the skeleton and from Comparative Anatomy than from the actual phenomena of the development of the plastron.

"If the plastron of the Chelonia were the homologue of the sternum in other Vertebrates," says Rathke, "one must also admit that the bones composing the shoulder and pelvis are situated in a manner entirely contrary to the general disposition of those parts in other animals." But that remark would equally apply as an argument against the carapace being homologous with the vertebræ and ribs, as Rathke contends it to be. It appears to me, however, that the peculiarly advanced position of the scapular arch in the embryo Chelonia, and, at its first appearance in all other Vertebrates, in relation to the thoracic hæmal arches,—a transitory relative position so beautifully explained by the recognition of the scapular arch as the hæmal arch of the occipital vertebra—equally explains and removes the anomaly of its position in regard to the plastron of the adult Chelonians as in regard to the carapace. In both instances the Chelonian peculiarity or anomaly, in the relative position of the bones of the shoulder, is due to the retrogradation of the scapular arch and the concomitant expansion of certain succeeding hæmal arches; as, for example, that formed by the second pair of dorsal ribs above, and by the episternal and hyosternal bones below; the one extending above the arch as a roof, the other beneath it as a floor. The discordance of the relations of the scapular and pelvic arches of the Chelonians with those in other Vertebrates no more necessitates the assumption that all the plastron belongs to the dermo-skeleton, than that all the carapace does.

With regard, indeed, to the relations of the pelvis to the plastron, whilst we should look amongst other Vertebrates, in vain, for instances in which the ossified exo-skeleton is developed beneath it, as Rathke supposes it to be in the *Chelonia* (fig. 8, in which *ps* and *xs* are referred by that author to the exo-skeleton†), we have not far

* *Loc. cit.*

† In the figures 8 and 9 *hh* are the hæmapophyses or abdominal ribs, specified in the *Chelonia* as *hs* hyosternals, *ps* hyposternals, and *xs* xiphisternals: 62 is the modified pleurapophysis called 'ilium'; 63 and 64, the modified hæmapophyses, called respectively 'ischium' and 'pubis'; 65, femur; 66, tibia; 67, fibula; 68, tarsus; 69, metatarsus and phalanges.

In a fossil *Emys* from Sheppey, described by Professor Bell, Sec.R.S., in our joint Monograph on the Fossil Chelonia of the London Clay, an intercalated piece is wedged in between the outer part of the interspace of the hyosternal and hyposternal on each side, like the dismemberments of the abdominal ribs at the outer part of that group of bones in the *Plesiosaurus*: and in another fossil Emydian from the same formation and locality, an intercalated bony piece extends across between the hyosternal and hyposternal on each side of the plastron. See Description of *Platemys Bullockii*, in my Report on British Fossil Reptiles, Report of the British Association, 1841, p. 164.

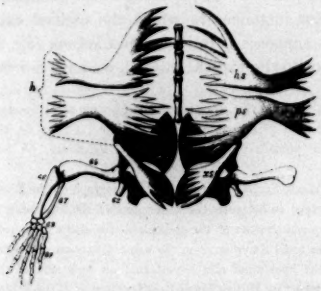
to seek for examples in which ordinary elements of the endo-skeleton are extended between the pelvis and the ventral integuments. Not to speak of the *Plesiosaurus* (fig. 7), in which the underlapping of the pubic-bones 64, by the abdominal ribs (h), might be due to displacement of the fossilized parts; the Puffins (*Fratercula*), the Guillemots (*Uria*) (fig. 9), and other common sea-birds of our coasts give the examples of the sternum prolonged backwards to beyond the vertical line traversing the acetabulum; whilst the hæmapophyses (h, h, ossified cartilages) of the three or four posterior ribs extend backwards beyond or as far as the most backwardly prolonged parts of the pelvis (68, 64), exactly in the relative position which the xiphisternals and hyposternals bear to the pelvis of the *Chelonia*.

In this comparison it is interesting also to discern the harmony which pervades the same vertebral elements in the characteristic forms which they assume in the same species. In the sea-birds cited the ossa pubis (64), or the hæmapophyses of the pelvic arch, are long and slender, like the costal hæmapophyses (h) beneath them: in the Chelonian reptiles the ossa pubis (64) are broad and expanded, like the costal hæmapophyses (ps, xs), which equally intervene between them and the ventral integument. Nature is ever liberal in rewarding with the perception of such harmonies whoever patiently investigates and rightly comprehends her arrangements.

Fig. 7.

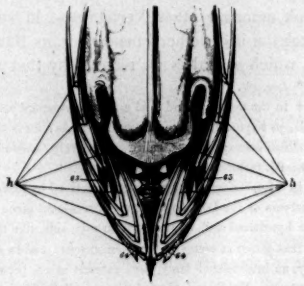
Relations of thoracic to pelvic hæmapophyses, Plesiosaurus.

Fig. 8.



Relations of thoracic to pelvic hæmapophyses, Chelonian.

Fig. 9.



Relations of thoracic to pelvic hæmapophyses, Bird (*Uria Troïle*).

Description of the Plate.

PLATE XIII.

(All the figures are of the natural size.)

Fig. 1. Embryo Turtle (*Chelone Mydas*).
1 *a*. Ossified parts of its carapace.
Fig. 2. Side view of the same embryo.
2 *a*. Side view of the ossified parts of its carapace.
Fig. 3. Front view of the same embryo.
3 *a*. Ossified rudiments of the plastron.
Fig. 4. External surface of the incipient carapace of a young Tortoise (*Testudo indica*).
Fig. 5. Internal view of the same carapace.
Fig. 6. External surface of the plastron of the same young Tortoise.
Fig. 7. Internal view of the same plastron.
Fig. 8. Ideal section of the same carapace and plastron.
The letters and figures are explained in the text.